PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

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OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. XXI. 1895–96.

THE 113TH SESSION.

GENERAL STATUTORY MEETING.

Monday, 25th November 1895.

The following Council were elected :—

President.

THE RIGHT HON. LORD KELVIN, LL.D., D.C.L., F.R.S.

Vice-Presidents.

Professor COPELAND, Astronomer-Royal for Scotland.
Professor JAMES GEIKIE, LL.D., F.R.S.
The Hon. Lord M'LAREN, LL.D.
The Rev. Professor FLINT, D.D.
Professor JOHN G. M'KENDRICK, LL.D., F.R.S.
Professor GEORGE CHRYSTAL, LL.D.

General Secretary—Professor P. G. TAIT.

Secretaries to Ordinary Meetings.

Professor CRUM BROWN, F.R.S.
JOHN MURRAY, Esq., D.Sc., LL.D.

Treasurer—PHILIP R. D. MACLAGAN, Esq., F.F.A.

Curator of Library and Museum—ALEXANDER BUCHAN, Esq., M.A., LL.D.

Ordinary Members of Council.

Dr ALEXANDER BRUCE, M.A., F.R.C.P.E.
Professor FREDERICK O. BOWER, M.A., F.R.S.
A. BEATSON BELL, Esq., Advocate.
Sir ARTHUR MITCHELL, K.C.B., LL.D.
Professor T. R. FRASER, M.D.
Dr ROBERT MUNRO, M.A.
Dr D. NOËL PATON, B.Sc., F.R.C.P.E.
C. G. KNOTT, Esq., D.Sc.
Sir W. TURNER, M.B., F.R.S.
Sir STAIR AGNEW, K.C.B.
Dr JAMES BURGESS, C.I.E., M.R.A.S.
JOHN S. MACKAY, Esq., LL.D.

By a Resolution of the Society (19th January 1880), the following Hon. Vice-Presidents, having filled the office of President, are also Members of the Council :—

HIS GRACE THE DUKE OF ARGYLL, K.G., K.T., LL.D., D.C.L.
SIR DOUGLAS MACLAGAN, M.D., LL.D., F.R.C.P.E.

Professor JAMES GEIKIE, F.R.S., Vice-President, in the Chair.

Chairman's Opening Address.

(Read December 2, 1895.)

I have to congratulate the Society on the re-appointment to our Presidential Chair of Lord Kelvin, first of British Physicists. Five years ago, as you are aware, Lord Kelvin became President of the Royal Society of London, at the urgent request of that body. This is not the first time, I may remind you, that we have provided a President for the London Society. The Philosophical Society of Edinburgh, as our body was known before it received the Royal Charter authorising it to assume its present designation, gave two Presidents to the Royal Society of London—namely, the Earl of Morton and Sir John Pringle, Bart.

In his Address, delivered two years ago, our respected ex-President took note of some of the more important papers communicated to the Society during the immediately preceding Sessions. I think I can hardly do better than follow his example. But before making the attempt, I wish briefly to refer to the successful completion within the present year of a great national undertaking. I allude, of course, to the famous "Challenger" Expedition. Our Library has now received the two volumes of *Summary of Results, with Appendices*, which complete the Society's set of the fifty volumes of Reports of the Scientific Results of the Voyage of H.M.S. Challenger. These voluminous Reports form by far the largest contribution hitherto made to Marine Zoology. Indeed, their publication marks an epoch in this department of science. Hardly less important are they as contributions to oceanography and cognate sciences, while they have at the same time added greatly to our knowledge of insular floras and faunas. As Editor and part Author of these Reports, and as Director of the work in connection with their publication, Dr Murray has had a laborious and engrossing task; and it may well be doubted

whether, with all his ability and energy, he could have brought the work to so successful a conclusion, had it not been for him a labour of love. In offering him their hearty congratulations, the Council are not without the hope that the manifest success which has attended the conduct and completion of the "Challenger" Expedition may weigh with H.M.'s Government, and induce them to accede to the proposal now being pressed upon them for a well-appointed Expedition to Antarctic Regions.

I would now turn attention to some of the work accomplished by Fellows during the past two Sessions. In the time at my disposal, it is obviously impossible to notice all the papers, which, for one reason or another, might be considered of importance. Some selection must be made, and perhaps I shall be pardoned if I limit attention very much to those which have most interested myself.

Dr Pole, in a notable paper, has given some account of the development of our knowledge of Colour-Blindness (from which he himself is a sufferer), beginning with the first well-authenticated case—that of Dalton—and coming down to the researches and discussions of Young, Helmholtz, Stokes, Clerk-Maxwell, and others. The Author analyses the relation of colour-blind sensations to those of normal vision, and discusses the import of certain variations found to exist in different colour-blind persons. Dr Peddie has also communicated to the Society some curious investigations in this interesting subject.

Dr Whiting read a paper on the Comparative Histology and Physiology of the Spleen, as found in twenty-two different kinds of animals, embracing all the typical vertebrates—from the fish to man. The paper is crowded with anatomical details, which evince long-continued and careful research. The general conclusion arrived at by the Author is that the giant-cells are not phagocytes. After careful study of the development of the nuclei of the young red-blood cells, he is opposed to the view that the extended nuclei are absorbed by the giant-cells of the spleen.

Dr Buchanan Young, having devised a new apparatus for counting Bacterial Colonies, has described and figured it in a communication to the Society. Dr Young is of opinion that the apparatus at present in use—that of Von Esmarch—is so far faulty

that it does not permit of the whole contents of the tube being actually counted, and thus gives only rough approximations. It was with the view of overcoming this defect that the Author devised his new apparatus. He has also contributed a paper on the Chemical and Bacteriological Examination of Soil, with special reference to the soil of grave-yards. He finds that the number of micro-organisms present in grave-yard soil exceeds the number present in virgin soil in a very marked degree.

Another subject relating to Public Health is treated of by Dr HUNTER STEWART in a paper on the Estimation of Carbon and Nitrogen in Organic Substances, by the Kjeldhal method, and its application to the analysis of potable waters. In this communication, the Author gives the results of the analysis of potable waters in Midlothian and the adjoining counties.

Great interest is now taken by the authorities in all matters relating to Public Health. Everyone who can look back for thirty or forty years must recognise the advance that has been made all along the line. But the reference to Dr Young's and Dr Hunter Stewart's investigations reminds me that even in these late days we often run the risk of being poisoned ourselves or of poisoning others by selecting sites for cemeteries and sewage-fields without first considering the geological structure of the ground. When we are disposing of decaying organic matter we should always ask ourselves the question—Do we know in what direction the natural underground drainage will transport the products of decay? In disposing of our dead and our refuse matter, unless we can answer that question without hesitation, we may be taking sure means for the propagation and dissemination of disease.

Professor T. R. FRASER has given us two most important papers on producing Immunity against Serpents' Venom, and on the Antidotal Properties of the Blood-Serum of the Immunised Animals. These communications are crowded with the details of varied and carefully-conducted experiments, and are of great scientific interest. Let us hope that the antidote described by the Author will be the means of reducing the great mortality that results from the bites of venomous serpents.

Professor RUTHERFORD has treated of the measurement of the

simple Reaction-Time for Sight, Hearing, and Touch. The reaction-times, he has found, differ from those of certain German observers. That for sight was mostly between 0·20 and 0·22 of a second; for hearing, from 0·15 to 0·16 of a second; for touch, from 0·1416 to 0·1906 of a second.

Dr JOHN MURRAY and Mr ROBERT IRVINE have discussed the occurrence of Oxides of Manganese in Fresh and Sea Water, with special reference to the lochs of the West of Scotland. They point out that manganese in combination with silica is present in many of the rocks within the drainage areas of those lochs, and that this, in all probability, is the source of the dioxide of manganese present in marine deposits. The silicates containing manganese are broken up, and the manganese converted into a bi-carbonate, which is eventually deposited as manganese dioxide.

Mr JOHN AITKEN, in addition to two interesting papers on the Hazing Effects of Atmospheric Dust, and on Breath Figures, gave an interesting communication on the number of dust particles in the atmosphere of certain places in Great Britain and on the Continent. He pointed out the remarkable fact that at Kingairloch and other places, when the sky was entirely clouded, the number of dust particles showed no tendency to rise as the day advanced; with a clear sky, however, the number of particles increased, and generally in proportion to the amount of clear sky. Much of the dust is so fine that it will scarcely settle of itself, but the deposition of vapour on the particles causes them to form rain-drops and fall, and so the air is purified.

Dr BUCHAN, in an elaborate paper, has insisted that the prevailing winds of the globe, in their direct and indirect effects, are the most powerful agents concerned in oceanic circulation. They originate and maintain the surface-currents of the ocean; and the influence of these currents is, through friction, felt to a depth of probably several hundred fathoms. In intertropical regions the prevailing trade-winds drive the surface-currents westwards towards the western shores of the continents, and there, accordingly, a greater depth of warm water is found in the upper layers of the ocean than elsewhere. Except where the rainfall is abnormally heavy, this water is not only very warm but has a higher salinity than the general average of the ocean. It is one of

the most remarkable results of the inquiry that these areas of high surface-temperature and high salinity are found represented at all depths down to the bottom of the sea, with just a tendency to extend with increase of depth. On the other hand, on the eastern sides of the oceans, whence the trade-winds start on their course, there is an upwelling of colder water from the greater depths towards the surface, in a manner similar to what Dr Murray, in a paper read some years ago to this Society, showed to occur in the case of the Scottish lochs when strong winds sweep over their surface. These cold areas of a lower surface-temperature and salinity are also continued down to the bottom, with a like tendency to expansion of the areas with descent. It is highly probable that the restriction of these respective areas to the same parts of the ocean for all depths would not have obtained if the depth of the ocean in comparison to its extent had not been so insignificant. The presence of ice-cold water at the bottom of the ocean in all latitudes implies a constant supply of water of a very low temperature from the Southern and the Antarctic Oceans, and, in a less degree, from the Arctic Ocean. This slow-moving current of cold water athwart the bottom of all parts of the ocean is effected, on the one hand, by the reduction in intertropical regions of the surface-waters by evaporation, and by the extra-tropical winds blowing polewards; and, on the other hand, by the greater specific gravities of the ocean in high latitudes together with the "head" of water accumulated there by the S.W. winds of the Northern and the N.W. winds of the Southern Hemisphere. There are subsidiary causes which powerfully influence oceanic circulation. Chief among these are abnormally heavy rainfalls—such as occur in the west of the Pacific; under-currents of high temperature and salinity from the Mediterranean and the Red Sea; the causes leading to the extensive upwelling seen in the Pacific to the south-east of the Sandwich Islands, and similar positions in the Atlantic and Indian Oceans; and the intertropical position of the line of lowest mean barometric pressure, resulting in a temperature much higher in the North than in the South Atlantic, and much higher in the South than in the North Pacific Ocean.

Mr A. J. HERBERTSON communicated a paper in continuation of his researches on Hygrometry, in which he describes the apparatus

used by him to measure the vapour in the atmosphere by weighing the water extracted from it when dried. Experiments were carried on at Ben Nevis Observatory, at Fort-William, and at Montpellier by Mr Herbertson and several assistants. The results obtained at these places agree so well that they have enabled the Author to construct tables showing the relations of vapour-pressure, atmospheric pressure, and temperature throughout a considerable range. He also discusses the reduction of the readings of the Dry and Wet Bulb Hygrometer, as given by different authorities, shows wherein these differ, and how far the present set of experiments throws light on this important question in practical meteorology.

Mr J. C. Beattie has given us a paper on the Relation between the Variation of Resistance in Bismuth in a Steady Magnetic Field, and the Rotatory or Transverse Effect. One important feature in this paper is the novel mode of experiment, the magnetisation being determined by the resistance change, so that the study of the Hall Effect is materially simplified. The anomalous behaviour of Bismuth is carefully studied under various conditions, and after being subjected to varied mechanical treatment.

This Society has, for many years, been particularly strong in mathematical and physical experimental science, and I think it will be allowed by those who have followed the work of the past two Sessions that our reputation in this respect has been fully maintained. Professor Chrystal has communicated a fundamental theorem regarding the Equivalence of Systems of Ordinary Linear Differential Equations, and its application to the Determination of the Order and the Systematic Solution of a Determinate System of Equations, and Professor Crum Brown has shown us, by means of a series of elaborate and ingenious models, how a Parallelopiped can be divided into Tetrahedra.

In his paper on the Kinetic Theory of Gases, Professor Tait pointed out, some years ago, that the celebrated equation of Van der Waals had been obtained by an unsound process, and thus could not be correct. He found it inconsistent with the results of his "Challenger" work on the compression of water under great pressure. In a recent paper in our *Proceedings* he returns to this question, basing his tests upon the lately-published results of Amagat's splendid work. The comparison is curious. From Van

der Waals' equation it is shown that the ratio of the pressures required to reduce the volume of a fluid by 10 per cent. and by 5 per cent. respectively, cannot lie between about 2·3 and 2·8. But Amagat's experiments show that, in all the seven liquids he examined (bodies as different as water, alcohol, and ether being included), the ratio in question varies from 2·5 to 2·73.

In another paper, Professor Tait deals with the effects of the Rotation of a Projectile on the Form of its Path. He shows that the main facts were known to Newton, and that he gave the correct explanation; that Robins, seventy years later, independently re-discovered, and gave beautiful experimental proofs of them, though, nowadays, Magnus alone is usually mentioned in connection with the subject. Professor Tait proceeds to show that the peculiar feather-like flight of a well-driven golf-ball, the initial upward concavity of its path, the length of time it remains in the air, and the consequent large increase of its range over that calculated from the ordinary theory of resisted projectiles, are due entirely to the rapid under-spin which the ball receives when properly struck. He also points out that, by sufficiently increasing the rate of rotation of a golf-ball, it may be made to move in a path which has a kink!

Dr SPRAGUE's paper, entitled a New Algebra, treats of some of the different kinds of operations which may be performed on permutations of the first natural numbers. It consists mainly of a development of the laws according to which the symbols denoting the seven operations combine with each other. It is found that these laws differ in several respects from the rules of ordinary Algebra, and any other calculus known to the Author, who therefore thinks that the somewhat ambitious title of the paper—a New Algebra—may be justified. It appears, from the examples given of the application of his method, that it is very closely connected with the theory of numbers; and one of the results obtained throws some light on the theorems of Fermat and Wilson.

Dr KNOTT's paper on Recent Innovations in Vector Theory is controversial in character. Its aim is to refute the arguments of certain critics who have asserted that some of the principles of Hamilton's Quaternions are unnatural, paradoxical, or lacking the characteristics of fundamental geometrical principles. The Author

also examines the several systems of vector analysis, which these critics believe to be superior to the quaternion system. His general conclusion is that the rival systems cannot compare in coherency, power, and flexibility, with quaternions, of which they are, at best, half-hearted imitations.

Professor Cayley, in a paper entitled Co-ordinates versus Quaternions, has said that, while co-ordinates are applicable to the whole science of geometry, quaternions seem to be a particular and very artificial method for treating such parts of the science of 3-dimensional geometry as are most naturally discussed by means of the rectangular co-ordinates. Professor Tait, on the other hand, holds that Cayley's views are only applicable to Hamilton's first conception of the quaternion, which was nothing more than a full development of imaginaries, but not to Hamilton's subsequent conception of the quaternion as an organ of expression, giving simple, comprehensive, and transparently intelligible embodiment to the most complicated of *real* geometrical and physical relations.

Amid his laborious, but happily hopeful, and so far successful work in developing the educational institutions of Cape Colony, Dr Muir has found time to continue his scientific researches. He has given us a paper on Sylvester's Problem in Elimination, in dealing with which he has found an axisymmetric determinant, which is the square of its own primary co-axial minors. In another paper, he has shown that the difference between any two terms of the adjugate determinant is divisible by the original determinant.

Although this Society is literary as well as scientific, we are not often favoured with literary communications. The only paper received during the past Session which might be classed as such is one by Professor D'Arcy Thompson on Ancient Symbolism. It is scientific, however, as well as literary—being an attempt to extract the hidden astronomical meaning "from graven emblem, from symbolic monument, from the orientation of temple-walls, from the difficult interpretation of Hellenic names of Hero and Heroine, of solar god and lunar goddess, of mysterious monster and fabled bird, of celestial river and starry hill."

During the last and the preceding Session we have been favoured with addresses from Professor Flinders Petrie, Dr Munro, and

Professor M'KENDRICK, which were delivered at the request of the Council. Professor Petrie's address was on a hitherto unknown race that held Upper Egypt between the VIth and XIth dynasties. In the first of Dr Munro's addresses, he discussed the Rise and Progress of Anthropology, while the second was devoted to an account of Lake-dwelling Research. Professor M'Kendrick's communication was on the Phonograph, with Experimental Illustrations.

We have also had addresses from the Duke of ARGYLL and Dr JOHN MURRAY—the former of whom treated of the glacial phenomena in the neighbourhood of Inveraray, and the latter of the remarkable and hitherto unknown forms of life dredged from the floor of the ocean during the famous voyage of the "Challenger."

Before concluding these very incomplete references to the work of the past two Sessions, I must not omit to mention Sir WILLIAM TURNER's careful review of the evidence adduced by M. Dubois as to the occurrence of a specimen of the long-looked-for "missing link." The remains described by Dubois consist of a calvaria, a femur, and two teeth, which were found embedded in certain ancient river-deposits. The bones are associated with other mammalian remains. According to Dubois, the skull-cap especially, and the other relics in a minor degree, show a union of simian and human characters; and he thinks he is justified in assigning them to a new genus—*Pithecanthropus erectus.* Sir William Turner, in his address, while admitting the low type of the skull-cap, was nevertheless of opinion that it is distinctly human, as also are the femur and the teeth. Since Sir William's paper was read, M. Dubois has visited Edinburgh, and brought with him the actual specimens. After carefully examining these most interesting remains, Sir William Turner admits that the skull is of a considerably lower type than the figures in M. Dubois' original paper had led him to believe. He still thinks, however, that the balance of evidence is in favour of its being human. It would seem that the British and Foreign anthropologists who have handled the specimens are divided in opinion. Many agree in considering that Dubois has found a "missing link"; some, again, think the skull is that of a large ape; while others maintain that it is simply that of a very

low type of man. As a geologist, I have no title to express any opinion as to the character of this remarkable calvaria, but I must say that I am impressed by the fact that the remains were obtained from deposits which seem to be of greater age than those which have yielded the earliest relics of man in Europe. According to Dubois, the deposits are not only of great extent and thickness, but they are charged with the relics of an extinct mammalian fauna. These remains have yet to be described; but from what I learned in conversation with M. Dubois, there can be but little doubt that they belong to a Pliocene horizon. Now, when one recalls the fact that the oldest skulls obtained from Pleistocene deposits in Europe—although not so small and ape-like as the presumably much older calvaria from Java—are nevertheless of a low type, one cannot but be much impressed by Dubois' discovery. It is, to say the least, highly suggestive that the oldest crania should have this character, and that the most ancient of all should be the most ape-like.

During the past Session we have added to our number fourteen Ordinary Fellows, and four British and six Foreign Honorary Fellows, but we have at the same time to lament the loss, by death, of ten Ordinary Fellows, and two British and three Foreign Honorary Fellows. Of these, the usual obituary notices will, in due time be communicated to the Society, but I may on this occasion be allowed to say a few words regarding each of our departed Associates.

Amongst the losses which come specially home to us as a Society is that of our former President, Lord Moncreiff. I regret that I can devote only a passing notice to his memory. But the duty of delineating his distinguished career as advocate and judge must be left to a more capable hand than mine. We were fortunate indeed in having him for our President, and I feel sure that I voice the opinion of our Society when I say that he most worthily occupied the Chair. The brilliant address which he delivered here, giving a "Review of the Hundred Years' History of the Society," and his charming Biographical Notice of the late Mr Campbell Swinton of Kimmerghame, are admirable alike in matter and manner. Lord Moncreiff was elected a Fellow of the Society in 1870.

Mr Patrick Dudgeon of Cargen, who died on 9th February 1895, at the ripe age of 78, combined in himself very happily the characters of savant and country gentleman. As a mineralogist he was a recognised authority, and few could equal his knowledge of the mineralogy of his native land. Amongst other scientific papers, he wrote a historical account of the finding and working of gold in Dumfriesshire. He also published numerous papers on the Place-names of Scotland, and compiled a Glossary of the Galloway Dialect. As showing his public spirit, I may mention that he recently established at Cargen a circulating library, which is free to all residents in the neighbourhood. He was much esteemed as a landlord, and endeared himself to his friends by his sterling character and kindliness. He was elected a Fellow of the Society in 1860.

Dr Thomas A. G. Balfour was educated at the High School and the University of Edinburgh. He at first studied for the ministry, entering the Theological Hall of the New College, but subsequently turned his attention to medicine, and having taken the degree of M.D., practised for forty years in this city. He wrote several works, of which the best known are, *All Nature a Symbol*, and *Christ's Jewels*, and he contributed a number of articles to periodicals. He died on 10th March of the present year, at the age of 70. He was elected a Fellow of the Society in 1870.

Dr Benjamin Carrington graduated as M.D. at Edinburgh, and subsequently became Medical Officer of Health for Eccles. He had a European reputation as a cryptogamic botanist, and was a copious contributor to scientific journals. His extensive Cryptogamic Herbarium is now in the Museum of Owens College, Manchester. He died on 18th January 1895. He was elected a Fellow of this Society in 1874.

The Rev. Thomas Turnbull was appointed to the first charge of the Established Church at Lesmahagow. He discharged his pastoral duties with much acceptance, and his sudden death will be long regretted by his parishioners, and especially by the poor, to whom he is said to have displayed "unbounded liberality." He died on 22nd May 1894, at the early age of 46—the same year in which he had been elected a Fellow of our Society.

Dr Murray Thomson was educated at Edinburgh University,

where he took the degree of M.D. in 1858. For some years he was Professor of Experimental Science in the Government Engineering College, Rourkee, and Examiner in Chemistry for the Government in the North-West Provinces of India. He obtained a gold medal from the University of Edinburgh for his thesis on "Sulphureous Mineral Waters," and was author of a Prize Essay on "The Mineral Waters of Scotland." He was also author of "Analytical Tables for the Use of Students of Medicine"; "Instruction in the Analyses of Waters and Cement and Limestones"; "Absorption by the Human Skin," and other treatises. He died on 16th January of the present year. He became a Fellow of the Society in 1863.

It is unnecessary to refer to the career of our much regretted Fellow, Dr Cleghorn of Stravithie, as Professor M'Intosh has already communicated to the Society a full and interesting sketch of his life.

Mr Peter Denny was born at Dumbarton in 1820. He was head of the well-known firm of William Denny and Brothers, which has launched over 500 vessels, representing upwards of 600,000 tons. The firm's shipbuilding establishment is admittedly one of the most complete and scientifically equipped in this country or elsewhere. Mr Denny was also the founder of the large engineering establishment of Denny & Co. In 1870 he was appointed one of a Parliamentary Committee on designs of ships of war. He was for some time Provost of Dumbarton, and, before the days of school-boards, maintained in that town a school for the children of the poorer classes. Subsequently he founded several bursaries to be competed for by pupils under the local School Board. He received the degree of LL.D. from Glasgow, and was decorated with several foreign orders of knighthood. He became a Fellow of this Society in 1876.

Mr Alexander Goodman More, in conjunction with Dr David Moore, published in 1866 an excellent account of the geographical distribution of plants in Ireland, under the title of "Contributions towards a Cybele Hibernica." He also prepared a list of the birds of Ireland in 1885, in connection with the Dublin Science and Art Museum; and subsequently published a capital guide to the Natural History Department of the same institution. He

succeeded Dr Carte as Curator of that Department, but after twenty years' service was compelled by illness to resign in 1887—the same year in which he became a Fellow of this Society.

Dr John Shand, after a distinguished University career, graduated in Medicine at Edinburgh in 1844. For some time he held resident appointments in the Royal Infirmary, and afterwards became Assistant Pathologist. Leaving Edinburgh, he practised with much success at Kirkcudbright, where he held nearly every public appointment open to one of his profession. He was very popular, and a welcome visitor in cot and country mansion alike. Like all the members of his family he was an accomplished, not to say a daring, horseman. He was fond of telling how, on a certain occasion, he astonished the Galloway Hunt with the performance of a nag which he had picked up for the not very extravagant sum of £10. Joining the hunt one day, he found his sporting friends balked by a formidable 6-feet wall, over which his wonderful purchase flew like a bird—giving a lead that none of the others dared to follow. In early manhood he was unfortunately prostrated by a hemiplegic attack. Recovering, he moved to this city, where he continued to practise his profession. He was a Fellow of the Royal College of Physicians, and was elected a Fellow of our Society so recently as last year. He died in the present year, on 12th March, at the age of 72.

Thomas Henry Huxley was born at Ealing, in Middlesex, on 4th May 1825. He was educated at Ealing School and Charing Cross Hospital, and served as Assistant-Surgeon on H.M.S. "Victory," and "Rattlesnake," from 1846–50. In 1854 he became Palæontologist to the Geological Survey, and Professor of Natural History in the Royal School of Mines, and, 1855, Fullerian Professor of Physiology at the Royal Institution. During his life he occupied many prominent posts. Thus in 1874 he was installed Lord Rector of Aberdeen University; was Rede Lecturer at Cambridge in 1883, and President of the Royal Society from 1883–1885. This is not the place, nor am I competent to give any account of Huxley's work as a man of science. He himself had a far too modest estimate of that work, when he wrote as follows: "I have subordinated any reasonable or unreasonable ambition for scientific fame which I may have permitted myself to entertain to other

ends:—to the popularisation of science; to the development and organisation of scientific education; and to the endless series of battles and skirmishes over evolution." Despite these modest remarks, we all know that his original investigations alone have given him a distinguished place among British biologists. He was elected a Fellow of our Society in 1876.

We have also lost the genial Professor Blackie. Born in Glasgow in 1809, he was appointed successively to the Chair of Latin Literature in Aberdeen, and to the Chair of Greek in Edinburgh. It has been said of him: "There was no winter in his year, no sorrow in his song. He was a perennial fountain of sweetness and light. There never came a frost keen enough to freeze the silvery column as it rose, nor a wind strong enough to break into drift its melodious fall." He contributed several papers to the *Transactions* of this Society; and his enthusiasm for the language of Greece in its ancient and modern phases gave a keen zest to the communications on the subject with which he favoured us. He became a Fellow of the Society in 1863, and died on 2nd March of the present year.

Arthur Cayley was born at Richmond, Surrey, on 16th August 1821. He graduated at Trinity College, Cambridge, in 1842, was called to the bar in 1849, and became Sadlerian Professor of Pure Mathematics at Cambridge in 1863. Those who are capable of appreciating his work say of him, that "while he was cosmopolitan in his mathematics, he was a master in every branch." Perhaps he will be best remembered as the creator of an entirely new branch of mathematics, by his discovery of the Theory of Invariants. The Royal Society's Catalogue contains the titles of 724 papers by Cayley, down to 1883. He became an Honorary Fellow of our Society in 1865, and died on January 26th of the present year.

James Dwight Dana was born on 12th February 1813 at Utica, U.S. of America. His appointment as mathematical instructor of midshipmen in the U.S. navy gave him opportunities of travelling which he used to the best advantage. He subsequently became Mineralogist and Geologist to the U.S. Exploring Expedition which was to circumnavigate the globe, under the command of Charles Wilkes. In the course of the voyage his party had to

take to the boats, and some hours afterwards they saw the vessel which had been their home for three years disappear beneath the waves. In 1845 he became Professor of Geology and Natural History at Yale College. Dana was a very hard worker and a prolific writer. Among his most important publications are the Reports of the Wilkes Expedition on Geology, Corals, and Crustaceans; his "System of Mineralogy," his "Manual of Geology," "Corals and Coral Islands," and "Characteristics of Volcanoes." He is the author of other manuals and of upwards of 200 separate papers. Dana was distinguished as an observer, but he also possessed in a high degree the power of generalising. His manuals of Mineralogy and Geology are probably as much used in Europe as they are in America. He died on 15th April 1895.

Sven Lovén was born at Stockholm in 1809, graduated as D. Phil. at the University of Lund in 1829, and after a year in Berlin (1830–31) devoted himself to the study of Zoology. He particularly investigated the marine fauna round the coasts of Scandinavia, and conducted the first scientific expedition to Spitzbergen. In 1841 he became Keeper of the Department of Lower Evertebrates in the State Museum of Natural History at Stockholm. He is the author of many scientific memoirs, the most important of which are published by the Swedish Royal Academy of Sciences. He was elected a Fellow of our Society in 1881, and died on 3rd September last.

Louis Pasteur, who died on 28th September last, was elected an Honorary Fellow of this Society in 1874. His whole career, as you are aware, has been characterised by a series of brilliant researches, to only a few of which can reference be made here. Some of his earliest work was successfully devoted to the structure of crystals. Subsequently he discovered the true nature of fermentation. Disproving the theory of Liebig, that fermentation was simply the chemical decomposition of bodies arising from the unstable equilibrium of their molecules, he showed that all types of fermentation are produced by microscopic organisms—bacteria being the chief agents by which the complex constituents of plants and animals are brought back to simple forms capable of serving again as food for plants. The nature of fermentation being known,

he was able, by a few simple directions, to put an end to the souring of wines and to the spoiling of beer and other products, and thus for the future prevented a loss of millions to his country. He next demonstrated that the belief in spontaneous generation was unfounded. As a direct result of Pasteur's early publications, we all know what a revolution in Surgery was effected by Sir Joseph Lister. Again, in the destruction that threatened one of the great industries of France—silk-worm raising—Pasteur came to the rescue. Owing to the "febrine" disease in the worms, the production had fallen from 57,000,000 lbs. to 8,000,000 lbs., when he discovered the cause of the disease and indicated the remedy, which has since restored the industry to its former prosperity. Pasteur's next triumph was over anthrax. At the time he began his researches, this disease had occasioned in the course of two years the death of 62,107 oxen, 534,245 sheep, and 2,196 horses. Having observed the fact that in man one attack of an infectious disease frequently renders an individual immune against a second, he prepared a vaccinating virus, capable of producing a mild form of anthrax in domestic animals, in the expectation that after recovery they would similarly be rendered immune. The experiments were crowned with success, and the resulting gain to agriculturists must be reckoned in millions of pounds sterling. Pasteur's treatment of hydrophobia proceeded on the same principles. In his Institute, several thousands of persons bitten by animals believed to be rabid, have been treated, and the number of deaths has been only a little over one per cent.

In concluding this imperfect retrospect, it only remains to congratulate the Society on its continued activity and prosperity. Zealous workers go to their honoured rest, and some of us that are left are no longer in the heyday of our strength; but our ranks are always being recruited by younger seekers after truth, who will doubtless maintain and increase the reputation of the Society in the years to come.

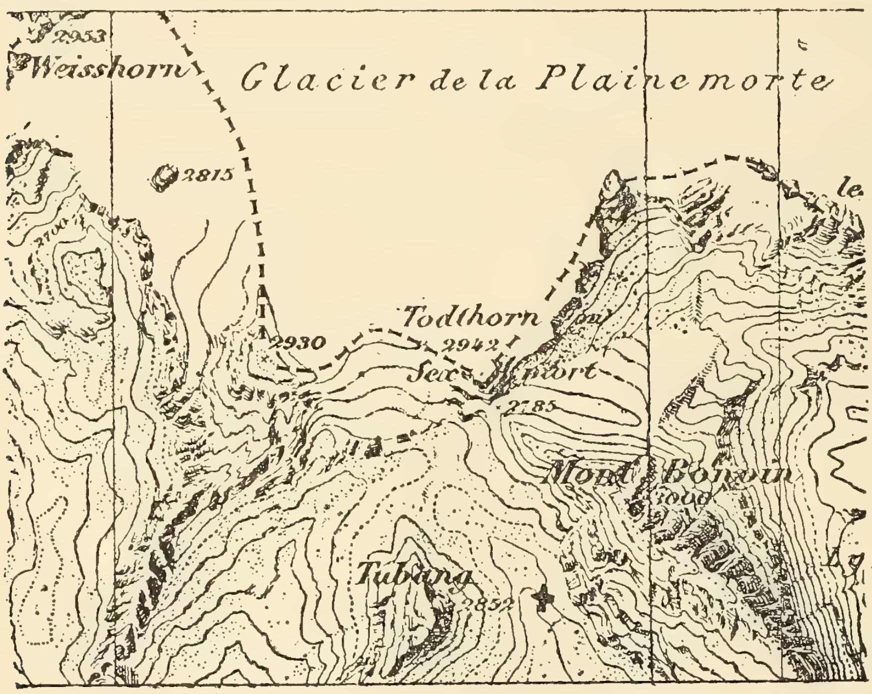
[The biographical notices and most of the short summaries of papers given in this Address were kindly prepared for me by our librarian, Mr Gordon.—J. G.]

Notes regarding, and Model of, Mass of Manufactured Iron, bearing date 1807, found beside Glacier de la Plaine Morte, Rhone Valley. By James Milne, Milton House Works.

(Read December 2, 1895.)

Mr President and Gentlemen,—During the past summer I visited for two or three weeks a new summer and winter Alpine resort, called Hotel du Parc, built by M. Zufferey and a partner near the village of Montana, on the north side of the Rhone valley, above Sierre, which is situated on a curious park-like plateau, with a number of lake-like reservoirs and quantities of timber, at an elevation of 5000 to 6000 feet.

While enjoying to the full the beautiful walks and views and



the superabundance of flowers, many of which are now getting comparatively scarce if not extinct in the older resorts, I pushed my way one day up to the Glacier de la Plaine Morte by a valley several miles to the eastward of the hotel. It is a stiff walk of about four hours from the hotel to the Glacier, first through thick pine woods and steep pasture, and latterly, as you penetrate the side valley, along very steep slides of debris composed of clayey slate and bottoms filled with very rough stones that have been

carried from the cliffs above, the only vegetation being the numerous ranunculi, gentiana bavarica, and other Alpine flowers familiar to those who are accustomed to search for them at these high altitudes.

I may here mention that we sprang a hen ptarmigan with a large brood of chicks; and a few days previously, when alone in the next valley, a grand eagle sailed over my head almost within gunshot. I never before found either in the Alps, although I have a number of times come across chamois within shot.

After visiting the Glacier and returning rather tired, as the day had been very sultry, I came upon this mass of iron. Naturally it attracted my attention, and I stopped to turn it over for examination; but being very tired I did not wait to take measurements, especially as a heavy thunderstorm had broken out and we were anxious to get home.

We had a native cowherd with us, and on questioning him, he at once made mention of the "chevalier" in connection with this iron. I jumped to the conclusion that he referred to Napoleon, and that this piece of iron was well known; but on further interrogation I found neither this man, nor apparently any other about the district, had ever heard about it, and the man's reference to the "chevalier" was simply that the block of iron might have been used by a shoeing smith.

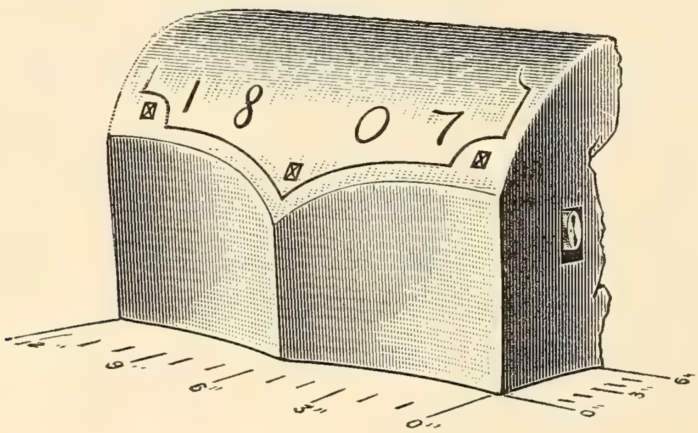
On thinking the matter over at home, I was satisfied that considerable interest might attach to this relic, so I determined to again visit the spot and take particulars, several others in the hotel also being desirous of seeing my find.

The morning fixed on for the expedition turned out wet, with thick mist, which reduced the party to Colonel Armstrong, R.E., and myself. We got thoroughly soaked at the start, and for the first two and a half hours I quite despaired of finding my way, far less finding such a small article among such a chaos of rocks and stones. However, the mist lifted for a few minutes, and I found my bearings had been correct, and I had struck the right side valley and about the right level, and soon after the day cleared up into brilliant sunshine and a nice breeze, which quickly dried our clothes.

I found the iron easily. It is lying quite close to the glacier

stream which runs down the valley, and must be buried deep under snow for the greater part of the year. There are few large stones about it. We crossed several deep patches of snow to reach the place. I show on the map the exact position in which the iron is lying, but it may have travelled for a long distance. I should be much afraid that next summer the snow and ice may have carried it into the bed of the stream, where it would be next to impossible to find it, although the place where it is lying is nearly level, and it may lie for some years before reaching the stream. The iron is not, however, more than fifteen feet from it just now.

On the mountain side, directly above, there are the remains of two small huts, a number of slabs of wood still lying about. The huts may be of recent erection, or they may be a hundred years old. Wood does not decay rapidly buried under snow, or in the clear



atmosphere of these high altitudes, and the huts may possibly have been dug out, built and roofed with slabs of slate, a hundred years ago. They are on a little knoll, and not in the way of avalanches.

The iron appears to me to be of fine quality. It has been battered up all along the top, which implies a certain amount of malleability, from which, and the rugged nature of the defective corner, I am inclined to think the percentage of carbon is low enough to suggest that it is wrought iron and not cast. I had a small file with me, and found the iron filed freely, and exposed a fairly homogeneous surface. The skin of oxide is very thin, and the file easily went through it. For all the time the iron has lain exposed, I do not think it has lost weight.

The model I lay before you is fairly accurate, as I took the various dimensions, and the rubbing I made with a piece of charcoal, picked up in the woods on my way to the spot, upon a piece

of paper taken for the purpose. The date, 1807, in hollow figures, is quite distinct, with a fragment of enclosing border line. There is a hole $1\frac{1}{2}$ inch by 1 inch right through, and filled up with a 1-inch bar and two iron spikes, the 1-inch bar having evidently been roughly cut off after being wedged in the hole. Perhaps this bar was fixed in position to enable the block to be carried easily. There is also a curious hole $1\frac{3}{4}$ inch by $1\frac{1}{4}$ inch in the bottom. You will see the border under the date, and three strange rough square indentations. The top has evidently been a good deal battered, and one end considerably staved over.

The opinion I at first formed was that the iron had been carried there by one of Napoleon's armies, which may have made a futile expedition up this valley, and abandoned some of the heavy baggage, the iron being part of its equipment. The unwieldy shape, however, does not support this theory, and the date is too late.

The opinion I now hold is that this iron has been used by some scientific expedition as a mark, or to determine the travel of the glacier or surface stone. I believe, about the year 1807, there was such an expedition carrying out research in the Alps, and I feel confident that some of the members of this or other kindred societies not only will be interested to hear of this find, but may possibly be able to give information which may lead to our discovering its origin.

The glacier does not come down this valley, but I have no doubt that in quite recent times it has extended over the edge of the rocks in this direction for a considerable distance. There is no vegetation in the bottom where the iron is lying, but there is a sparse growth of various ranunculi, &c., a short distance up the sides of the valley, perhaps 100 to 150 yards above.

The weight of the iron I estimate at from 250 to 300 lbs.

On the Relationship of the Liver to Fats. By D. Noël Paton, M.D., F.R.C.P.E., *Superintendent of the Research Laboratory of the Royal College of Physicians, Edinburgh.*

(Abstract of a paper appearing *in extenso* in the *Journal of Physiology*, February 1896.)

(Read December 2, 1895.)

The liver is on the direct channel of absorption of carbohydrates and proteids, and its connection with the metabolism of these substances has been fully demonstrated.

It is not on the channel of absorption of fats, and its relationship to the metabolism of fats has never been systematically investigated.

The object of the present research is to elucidate this question.

The first part of the paper is concerned with an investigation into the methods of estimating the fats. It is shown that after complete extraction with ether, by treating the residue with hydrochloric acid and heat, a further amount of material may be extracted by ether, but that it is impossible to say how far this is composed of the fatty acids previously combined with bases as soaps, and how far from the decomposition of nuclein bodies. The present research, therefore, is concerned simply with the fats extracted by ether.

It is next shown that the ether extract is no fair measure of the amount of fats present, since it contains on an average only 57 per cent. of fatty acids. In estimating the amount of fats, it is necessary to determine the fatty acids, and not merely the ether extract. The fatty acids amount to between 2 and 3 per cent. of the liver substance.

In the liver the proportion of palmitic and stearic acids to oleic acid is shown to be markedly higher than in the fats of the rest of the body. Hence the liver fat has a high melting-point.

As regards the nature of the compounds of these fatty acids, it is found that one of the most important is lecithin. About one-half of the acids is usually combined in this substance. About 2·3 per cent. of the liver, or 10 per cent. of its dry solids, is com-

posed of lecithin. When fat accumulates in the liver, the proportion of lecithin falls. In connection with the function of this hepatic lecithin, the part played by lecithin in the egg, as the store material for the phosphorus of the embryo, is pointed out, and it is concluded that the function of the hepatic lecithin is to fix phosphorus as a step towards its synthesis into the nucleo-compounds of the body.

The possibility of determining the amount of free fatty acids in the ether extract of the liver is considered, and it is shown that Hofmann's conclusions are valueless.

The amount of cholesterin is found to be smaller than that indicated by Kausch: in rabbits there is an average of 0·039 per cent., and in cats 0·029 per cent. of the liver. In estimating the fatty acids, the cholesterin may thus be disregarded.

The percentage of ether extract not combined with fatty acids is shown to vary greatly. On an average it amounts to 10 per cent. The nature of this is not considered.

Taking the fatty acids as a measure of the amount of fats, the influence of various conditions on the liver fats is investigated.

A. *Starvation.*

It is shown that after three days' starvation in cats, and after four days' in pigeons, the average amount of fatty acids in the liver is maintained. The importance of this in fixing waste phosphorus as lecithin as a step to its reconversion to nucleo-compounds is indicated.

B. *Influence of various Foods.*

1. *Fats.*—That the fats of the food are largely stored in the liver of certain animals is shown by experiments on cats and rats. That when so stored they are afterwards (in cats in about seventy hours) either metabolised in the organ or exported from it is also shown. That the former process goes on was indicated in considering the function of lecithin.

2. *Carbohydrates.*—The generally accepted statement that the amount of glycogen and fat corresponds is disproved. Food rich in carbohydrates is shown to increase the liver fats. The question of the source of these fats is investigated, and it is shown that as

the glycogen disappears from the liver there is an *actual* increase in the liver fats. That this is not a transport of body fats to the liver is indicated by the fact that the blood serum remains clear, and that the melting-point of the liver fats does not fall, as would be expected if the body fats with their lower melting-point were added. It is, therefore, concluded that the fats are formed from the glycogen.

3. *Proteids.*—The evidence of the production of fats from proteids is considered. The evidence of the proteids of the food being a source of fat in the liver is investigated. The evidence opposes the view that liver fats are formed from proteids.

On the Cause and Nature of the Chemical Changes occurring in Oceanic Deposits. By W. N. Hartley, F.R.S., Royal College of Science, Dublin.

(Read December 16, 1895.)

In *Nature* of January 24, 1895, appears an abstract of a paper read before the Royal Society of Edinburgh on March 7, 1892, by Dr John Murray and Mr Robert Irvine, and published in the *Transactions* of the Society, vol. xxxvii. part 2, No. 23, entitled "Chemical Changes between Sea-water and Oceanic Deposits."

This is an account of a chemical examination of the sea-water salts in the water adhering to or retained in mud, with special reference to the formation of the deposit known as "Blue Mud."

Dittmar's analysis of sea-water is quoted and compared with an analysis of mud-water. The chief points of difference between the two is the occurrence in mud-water salts of 0·206 per cent. of ammonium sulphate, 0·729 per cent. of magnesium carbonate, and 0·18 per cent. of manganous carbonate; also that the total salts are low in proportion to the chlorine they contain.

The occurrence of ammonium sulphate in this mud, and also of manganous carbonate, are facts of much interest; but there are some equations given to explain the chemical changes which the mud undergoes which are not strictly in accordance with facts. There are three points which I would desire to draw attention to: first, the reduction of the sulphates; second, the oxidation of sulphuretted hydrogen; and third, the formation of manganous carbonate.

The equations are written without reference to the part played by water in the chemical changes involved, but it may have been thought that the accuracy sacrificed was compensated by the simpler form of the equations.

Equation No. 1 reads as follows:—

$$RSO_4 + 2C = 2CO_2 + RS.—$$

where R is an alkaline earth metal.

This, as a matter of fact, expresses what takes place in the "black-

ash" furnace at a very high temperature in the course of Leblanc's process for the manufacture of soda; and the transformations which take place in the "alkali waste" or refuse from this process, under the influence of atmospheric agencies, are, perhaps, worth studying in this connection.

Thus, in the case of calcium sulphate its reduction in this fashion produces calcium sulphide, a compound which is insoluble in water. Calcium sulphide is the principal constituent of the "black-ash" waste, after the carbonate of soda has been dissolved out; and when this is left in contact with water it gradually decomposes, yielding calcium sulphydrate and calcium hydroxide thus:—

(Kraushaar, *Dingler's Polytechnisches Journal*, vol. 226, p. 412.)

$$2CaS + 2H_2O = CaH_2S_2 + CaH_2O_2 .$$

The action of air upon the calcium sulphydrate has been shown by the same author to form calcium polysulphide and calcium thiosulphate, CaS_2O_3. The action of carbon dioxide is well known.

The sulphur in the mud when the sulphates have been reduced is supposed by Messrs Murray and Irvine in part to pass into solution as sulphuretted hydrogen, and then to be oxidised back to sulphuric acid, which in turn decomposes the carbonates and re-forms sulphates. "A certain part of the sulphides, or it may be of the hydrosulphuric acid, reduces the ferric oxide of the deposit, forming sulphide of iron." The sulphide of iron, it is pointed out, gives the characteristic blue-black colour to the great majority of blue muds.

Now, it is a well-known fact that living organisms reduce the sulphates and the mud becomes more or less black or dark in colour by reason of the formation of ferrous sulphide. Hence, those who are accustomed to examine rivers and streams for pollution first take note of the colour of the mud, and observe whether it undergoes any change, for when the pollution is intermittent the mud may be red or yellow one day and black the next, subsequently returning to its original colour. The chemistry of the changes which take place between the constituents of such muds has in a great measure been explained by Mr J. Y. Buchanan in a paper "On the Occurrence of Sulphur in Marine Muds," etc. (*Proc. Royal Soc. Edin.* Read December 1, 1890).

He points out the part which is played by manganese sulphide,

and shows that it acts like an alkaline sulphydrate in converting ferric hydroxide into ferrous sulphide. When air acts on the ferrous sulphide, sulphur separates according to a well-known reaction, and ferric hydroxide is re-formed.

In the absence of manganese it is doubtful whether a similar action takes place through the agency of calcium sulphate being reduced to calcium sulphydrate, as will appear from a careful study of the following facts which have already been ascertained.

It was shown by Lothar Meyer, as far back as 1864, that the sulphuretted hydrogen in mineral waters is very generally a product of the algæ which are found in them (*J. für Praktische Chemie*, vol. xci.). Cohn described a bacterium found in a well-water which evolved sulphuretted hydrogen (*Beiträge zur Biologie der Pflanzen*, vol. i. part 3, 1875). Etard and Olivier showed that in mineral waters sulphate of lime yields sulphuretted hydrogen, or even free sulphur (*Comptes Rendus*, 1882). Hoppe-Seyler described the fermentation of cellulose (*Zeitschrift für physiologische Chemie*, vol. x. part 5, 1886) by the agency of the *bacillus amylobacter*, and showed that the organism resolves the cellulose into equal volumes of methane, CH_4, and carbon dioxide, CO_2. When, however, there were sulphates present in the water the volume of carbon dioxide was greatly increased, being ten volumes of CO_2 to one volume of CH_4. This arises from the oxidation of the methane by the sulphate in accordance with the following equation :—

$$CH_4 + CaSO_4 = CaCO_3 + H_2S + H_2O.$$

Sergius Winogradsky has made a most careful investigation of the life-history of various species of sulphur-bacteria or beggiatoæ (*Botanische Zeitung*, pp. 490–610, 1887).

These organisms separate sulphur from sulphuretted hydrogen, and this gas is quite indispensable to their life; but they cannot live in a saturated solution of H_2S, as was believed by Cohn.

The sulphur which the beggiatoæ separate from H_2S is oxidised by them into sulphuric acid, and in this way the carbonates formed by the fermentation of cellulose accompanying the reduction of sulphates and in presence of sulphuretted hydrogen are subsequently decomposed according to the two equations :—

$$(1)\ CaSO_4 + CH_4 = CaCO_3 + H_2S + H_2O.$$
$$(2)\ CaCO_3 + H_2S + 2O_2 = CaSO_4 + CO_2 + H_2O.$$

These sulphur-bacteria are able to subsist on a very minute quantity of organic matter, and the only product of their "respiration" is sulphuric acid. This substance is formed by a process which is termed respiration, because it is the cause of a chemical change which sets free energy in the form of heat. The sulphur which the bacteria remove from sulphuretted hydrogen is secreted by the organism, and subsequently oxidised.

As a matter of fact, the deoxidation of sulphates contained in either fresh or sea water is due to the action of such organisms as the *bacterium sulphureum* and the *bacterium hydrosulphureum*.

In the Black Sea, as we know from the paper read by Dr Andrussow, a large amount of sulphuretted hydrogen is dissolved in the water. I certainly understood Dr Andrussow to say, at the meeting of the British Association at Edinburgh, that the process whereby sulphuretted hydrogen is formed is due to the bacteria, and I immediately connected this statement with the known reduction of calcium and magnesium sulphates in both sea and fresh water by the sulphur-bacteria or beggiatoæ.

But it appears, in the translated abstract of his paper in the *Journal* of the Royal Geographical Society, January 1893, to be attributed to the decomposition, after death, of a great number of minute organism and river débris. It is well known that the organic materials of dead organisms cannot decompose except by the operation of living organisms (Pasteur, *Comptes Rendus*, vol. 56, p. 738). In Dr Andrussow's diagram there are the following data :—

"Lower Limit of Organic Life,	100 fathoms.
Upper Limit of H_2S,	100 fathoms.
Upper Limit of Sulphides,	200 fathoms.
No organic life except bacteria."	

This certainly appears to bear out my interpretation of his remarks. There is, however, a note to the effect that the generation of H_2S and of sulphides goes on down to a depth of 800 fathoms.

It is now of no importance in what precise manner his remarks

were to be interpreted, because, as a matter of fact, the actual organism which is the cause of this change has been described and named by Zelinsky as the *Bacterium Hydrosulphureum Ponticum* (*Proc. of the Russian Physical and Chemical Society*, vol. xxv. part 5, 1893; also *P. and G. C. Frankland's Micro-Organisms in Water*, p. 458).

This bacterium appears to live upon the cellulose of vegetable débris, and it abstracts the oxygen from sulphates.

I must take exception to Messrs Murray and Irvine's explanation of the change which they believe takes place in solutions of sulphuretted hydrogen. They cite experiments to show (1st) that a solution of sulphuretted hydrogen in pure water, when left to stand for a month, was found to be oxidised to sulphuric acid; (2nd) that a solution of sulphuretted hydrogen in sea-water was oxidised to sulphuric acid.

To take this latter case first, the oxidation is caused by the bacteria in the manner already explained. In the first case, where the sulphuretted hydrogen is dissolved in pure water, there is, according to my experience, no oxidation of sulphuretted hydrogen. In January of the present year I had six wide-mouthed bottles charged with a litre of distilled water, which filled each of them to about $\frac{2}{3}$ its capacity. In succession they were charged to saturation with carefully purified hydrogen sulphide, evolved from ferrous sulphide by the action of sulphuric acid. These bottles were kept in a fume chamber in full daylight, where the morning sun could reach them. The temperature varied from 12° to 17° C. In five or six days it was found that the bottles did not smell of sulphuretted hydrogen. In two of them there had been a slight deposition of sulphur. It was thought that the oxidation of the sulphuretted hydrogen might not necessarily have formed sulphurous or sulphuric acid, but one of the thionic acid. Accordingly, a very careful examination was made of each liquid. The introduction of a little cupric chloride solution gave no black precipitate. Iodine solution was not decolorised. Barium chloride gave no precipitate. Other experiments were made, but they need not be referred to, as no evidence was obtained of any oxidation products being contained in the liquid. The gas seems to have simply diffused out of the vessels.

It was then determined to ascertain whether ferric oxide in presence of oxygen would oxidise sulphuretted hydrogen solution. Ignited ferric oxide, also some red hæmatite, were placed in a Lunge nitrometer with a solution of sulphuretted hydrogen, and mixed with this was some oxygen, but no action took place; the gas did not diminish in volume. When ferric oxide is replaced by ferric hydroxide, oxidation takes place with great rapidity.

It follows, then, that sulphuretted hydrogen is not oxidised to sulphuric acid simply by the oxygen of the air, and ferric oxide is not acted upon by sulphuretted hydrogen so as to produce ferrous sulphide and free sulphur, according to Dr Murray and Mr Irvine's equation :—

$$(4)\quad Fe_2O_3 + 3H_2S = 2FeS + S + 3H_2O.*$$

For such action to take place it is necessary that the ferric compound should be the hydroxide, such as is used in the purification of coal-gas.

The thermo-chemical equations worked out for the chemical changes which take place between the organic matter and the mineral salts in mud, show that, at each stage, there is heat evolution :—

The fermentation of cellulose.—The composition of cellulose, and also of starch, is $x(C_6H_{10}O_5)$; where x is a whole number, probably not less than ten. The first change in all such carbohydrates is hydrolysis, or the simple addition of the elements of water to the molecule or fraction of the molecule represented by $C_6H_{10}O_5$; accordingly we have cellulose and water represented by $C_6H_{10}O_5 + H_2O$.

Heat of Formation.

(Berthelot's Tables in Salet's *Agenda du Chimiste.*)

		Heat Units.
C (diamond) $+ O_2$	$= CO_2$ (gas)	$+ 94{\cdot}3$ †
C (diamond) $+ O_2$	$= CO_2$ (dissolved)	$+ 99{\cdot}6$

* The above action can take place only at a high temperature, as, for instance, in the Claus kiln, but in the working of this kiln, by the judicious admixture of air, the formation of FeS is prevented, the Fe_2O_3 remains unchanged and sulphur is separated.

† C (amorphous) yields $+97{\cdot}6$ units.

		Heat Units.
$Ca + O$	$= CaO$ (solid)	$+132$
$Ca + O$	$= CaO$ (dissolved)	$+150{\cdot}1$
$CaO + H_2O$	$= CaH_2O_2$ (solid)	$+15$
$Ca + C + O_3$	$= CaCO_3$ (solid)	$+269{\cdot}6$
$CaO + CO_2$	$= CaCO_3$ (precipitated)	$+19{\cdot}6$
$Ca + S + O_4$	$= CaSO_4$ (solid)	$+320$
$CaO + SO_3$ (solid hydrate)	$= CaSO_4{\cdot}2H_2O$ (solid salt)	$+49{\cdot}4$
$S + O_3$	$+ H_2O = H_2SO_4$ (solution)	$+141$
$S + H_2$	$= H_2S$ (gas)	$+4{\cdot}6$
$S + H_2$	$= H_2S$ (solution)	$-9{\cdot}2$
$H_2 + O$	$= H_2O$ (liquid)	$+69$
$x(C_6H_{10}O_5)$	$=$ cellulose	$x(+228{\cdot}8)$

Precipitated

H_2SO_4 (dissolved) $+ CaH_2O_2$ (dissolved) $= CaSO_4{\cdot}2H_2O + 31{\cdot}2$

Calcium sulphate in solution, or precipitated, has a heat of formation derived from $S + O_4 + Ca = 141 + 150{\cdot}1 + 31{\cdot}2 = 322{\cdot}3$ heat units.

Fermentation of Cellulose and Starch.

(1) $C_6H_{10}O_5 + H_2O$ (liquid) $= 3CH_4 + 3CO_2$
$+ 3(18{\cdot}9) + 3(94{\cdot}3) - 228{\cdot}8 - 69$
Total $= +41{\cdot}8$ kilo. heat units.

The Reduction of Calcium Sulphate.

(2) $CaSO_4$(dissolved or precipitated) $+ CH_4 = CaCO_3 + H_2S + H_2O$
$+ 269{\cdot}6 + 4{\cdot}6 + 69 - 322{\cdot}3 - 18{\cdot}9$
Total $= +2{\cdot}0$ kilo. heat units.

(*a*) H_2S (solution) $+ O = H_2O$ (liquid) $+ S$ (solid) $+ 69 + 9{\cdot}2$
Total $= +78{\cdot}2$ kilo. heat units.

(*b*) S (solid) $+ 3O + H_2O$ (liquid) $= H_2SO_4$ (solution) $+ 141$ kilo. heat units.

Equations (1) and (2) represent changes which take place concurrently, in both cases with heat evolution.

If the methane in (1) is oxidised as in (2) concurrently with its production we have a total heat evolution $+47{\cdot}8$ units. The gaseous H_2S being evolved, its heat of formation is $+4{\cdot}6$ heat units, but it soon goes into solution, and its heat of formation is

then a minus quantity $-9{\cdot}2$ units, and the thermal effect in (2) would result

$$-341{\cdot}2 - 9{\cdot}2 + 269{\cdot}6 + 69$$
$$= -350{\cdot}4 \quad + 338{\cdot}6$$
$$\text{Total} = -11{\cdot}8 \text{ heat units absorbed.}$$

This is, however, in respect to only one molecule of CH_4, and there are three molecules involved in equation (1), and therefore, if we consider the two changes taking place simultaneously, we have $+41{\cdot}8 - 35{\cdot}4 = +6{\cdot}4$ heat units evolved. So that, whatever may be the condition of the sulphuretted hydrogen liberated, the ultimate effect is heat evolution.

Let us take into account the oxidation of the sulphur and of the sulphuretted hydrogen; presuming that the latter gas is in solution, then by equations on the preceding page:—

(*a*) $$\underset{-9{\cdot}2}{H_2S \text{ (solution)}} + O = \underset{+69}{H_2O \text{ (liquid)}} + S \text{ (solid)}$$

Heat evolved $= 78{\cdot}2$ heat units.

(*b*) $$S \text{ (solid)} + 3O + H_2O \text{ (liquid)} = H_2SO_4 \text{ (solution)}$$

$+141$ heat units.

Total heat evolution in (*a*) and (*b*)

$$78{\cdot}2 + 141 = 219{\cdot}2 \text{ units.}$$

We know that this is the course of chemical effect by the life processes of some, if not all, of these sulphur-bacteria; but let us suppose that there may be some organisms which act directly upon sulphuretted hydrogen in presence of oxygen without the separation of sulphur, it will be seen that the heat evolved under such conditions is nothing like so great as in the former case:

(*c*) $$\underset{-9{\cdot}2}{H_2S \text{ (dissolved)}} + 2O_2 = \underset{+141}{H_2SO_4 \text{ (dissolved)}}$$

Heat evolved $= 150{\cdot}2$ units.

The difference between the changes in (*a*) and (*b*), taken together, as compared with (*c*), results in a difference of 69 heat units in favour of the former, and it is therefore in the highest degree probable that the former chemical change is actually that which takes place.

I will now refer to the occurrence of manganese carbonate in the mud-water, which was found to amount to 0·18 per cent., although no manganese was found in the sea-water.

In a paper by Mr W. E. Adeney, in the *Scientific Proc.*, Royal Dublin Society, April 1894, a very lucid account is given of the changes which take place in sludge derived from sewage which had been submitted to the action of permanganate.

It was found that the manganese dioxide which appears to be the first product of the reducing action of the organic matter became converted into manganese carbonate whether in presence of much water or not. The author suggests that the reduction and conversion into a carbonate of such a stable oxide is due to a similar action to that whereby Gayon and Dupetit found nitre to be converted into nitrogen and potassium carbonate, under the influence of two micro-organisms isolated from sewage and named the α and β *bacterium denitrificans.*

It is supposed that the available oxygen in the manganic oxide is used up by an organism in destroying the carbon of organic matter, and by thermo-chemical equations it is shown that this can be a considerable source of energy to the organism. It is extremely probable that the manganese in this case furnishes oxygen to such an organism as the *bacillus amylobacter*, and is utilised in the destruction of cellulose, or in destroying the methane which is evolved during the fermentation of this substance.

Conclusions.

1. The sulphates in fresh and sea water mud are reduced to carbonates and sulphuretted hydrogen by the fermentation of cellulose through the agency of different species of bacteria.

2. We have no evidence whatever that calcium sulphide or calcium sulphydrate is formed in the course of the chemical change, and therefore the equations 1, 2, 3 of Messrs Murray and Irvine are not to be relied on for an exact explanation of the reactions which occur in mud-waters.

$$(1)\ RSO_4 + 2C = 2CO_2 + RS.$$
$$(2)\ RS + 2CO_2 + H_2O = H_2S + RCO_3CO_2.$$
$$(3)\ RS + RCO_3.CO_2 + H_2O = 2RCO_3 + H_2S.$$

3. Sulphuretted hydrogen is not oxidised simply by atmospheric air to sulphuric acid at ordinary temperatures.

4. Sulphur-bacteria secrete sulphur which they separate from sulphuretted hydrogen, a compound which is essential to their life. This sulphur is then oxidised by the bacteria to sulphuric acid, which acts upon the carbonates and again forms sulphates.

5. The formation of manganese carbonate appears to be due to an action on manganic oxide similar to that which operates on sulphates in presence of cellulose undergoing fermentation.

Note.—The process of sulphur fermentation was first noticed by me in 1870. It occurred in stagnant water which filtered through a bed of new red sandstone after it had passed through mud highly charged with vegetable débris. In 1872, when studying the life-history and chemical changes caused by the *bacteria*, called at that time *vibrios*, I found that beer was capable of undergoing a very powerful sulphur fermentation. It was not then suspected of being due to bacteria, but believed to be the effect of an abnormal fermentation caused by the common forms of yeast (*saccharomyces*). On the publication of Cohn's paper in 1875, giving an account of a bacterium found in well-water, which evolved sulphuretted hydrogen, an explanation was afforded capable of application to all the facts known at the time.

It may easily be shown by thermo-chemical equations that neither organic matter nor carbon itself can reduce sulphates such as calcium sulphate to sulphides except at a high temperature, because the thermal effect results in an absorption of heat under the most favourable conditions for such chemical changes.

W. N. H.

February 13th, 1896.

On Chemical Changes in Marine Muds. By Robert Irvine and John Murray, LL.D.

(Read January 20, 1896.)

On 16th December 1895 a paper written by Professor W. N. Hartley, F.R.S., Royal College of Science, Dublin, was read before this Society "On the Cause and Nature of Chemical Changes occurring in Oceanic Deposits," in which he criticised and took exception to certain conclusions we had arrived at, which formed the basis of a paper read by us in March 1892, and published in the *Transactions* of the Society, Vol. xxxvii., Part ii., No. 23, under the title "On the Chemical Changes which take place in the Composition of Sea-water associated with Blue Muds on the Floor of the Ocean."

At the time of writing his paper Professor Hartley had not seen ours, and was only in possession of the necessarily curtailed notice of it which appeared in *Nature* of January 24, 1895, from which he appears to have drawn inferences that a fuller knowledge would have altered. Practically, with the exception of one or two of our experimental results, Professor Hartley is in accord with us so far as the principal results are concerned. In taking exception to the formulæ by which we endeavour to explain the decomposition of sulphates in sea-water in the presence of organic matter and ferric-hydrate, or ferruginous clay, he assumes the equations we use to mean the same as that which would occur in a black ash furnace under the influence of great heat. Our object in adopting the explanation of the reaction which appeared in our paper was to render it as simple as possible; and occurring as it does in mud saturated with water, we did not think it necessary to specially refer to the hydrous condition of the salts.

The reduction of sulphates in mineral water by organic matter is so familiar to chemists that we need not refer further to it, except that it is acknowledged that most waters holding in solution sulphides or sulphuretted hydrogen derive these from the deoxidation of the sulphates of the alkali or the alkaline earth present in the water. Of course, it was a reaction similar to this

that we referred to in the formulæ given in our paper. (See vol. xxxvii. p. 496.)

Professor Hartley next refers to the influence of living organisms reducing sulphates and forming with ferric-hydrate ferrous-sulphide, founding his facts upon a paper by Mr J. Y. Buchanan "On the Occurrence of Sulphur in Marine Muds." It is not necessary for us to further reply to this statement, as the subject has been dealt with, first, in a paper by Irvine and Gibson,* and, second, in a paper by Murray and Irvine,† showing that whilst manganous sulphide is rapidly decomposed by carbonic acid, ferrous-sulphide is stable under such circumstances; so that whilst ferrous-sulphide is present in these blue muds, often in large quantity, manganous sulphide is decomposed by the carbonic acids or bi-carbonates present in sea-water, bi-carbonate of manganese being formed and sulphuretted hydrogen given off.

Professor Hartley goes on to state that alga or cellulose has the power of decomposing sulphate of lime and producing sulphuretted hydrogen, and that this decomposition is effected through the agency of the "bacillus amylobacter," giving the somewhat complicated formula described by Hoppe-Seyler in 1886, by which methane and carbon-dioxide are produced under the influence of this bacterium. We do not think it necessary to criticise the results he refers to, as in our paper we admit and refer to the influence of bacteria in bringing about the changes which lead to the production of blue muds. Indeed, experimentally we showed in 1892 that sea-water containing sterilised organic matter and ferric-hydrate failed to exhibit the changes which in an unsterilised condition gave the characteristic reduction of the sulphates and the production of ferrous sulphide, such as occurs in blue muds. In Frankland's book on *Micro-Organisms in Water*, page 114, is given a table by Russell showing the number of bacteria found in sea muds deposited at different depths. In mud deposited at a depth of 15 metres, 1 c.c. contained 245,000, whilst the water immediately above only contained 121,000. The numbers are rapidly

* "Manganese Deposits in Marine Muds," Irvine and Gibson. *Proceedings Royal Society of Edinburgh*, 1890 and 1891.

† "Manganese Oxides and Manganese Nodules," Murray and Irvine. *Transactions Royal Society of Edinburgh*, Vol. xxxvii., Part iv., No. 22.

decreased in muds deposited at greater depth: thus at 500 metres 1 c.c. of mud contains only 12,500 bacteria, whilst the water above contains only 22 per c.c. Russell's investigations seem to show that the number of microbes decrease in a marked degree in the cooler waters of the Temperate Zone, whilst in the Mediterranean muds or slimes, where the water is warm even at great depths, they occur in very large proportion.

It is to be observed that besides the organic matter carried to the sea from continents, and alga, one of the products of the sea, the remains of nitrogenous organic matter and effete products of animal organisms bulk largely in the fermenting or decomposing mass, and it is not at all the simple cellulose or alga with which we have to deal, as Professor Hartley argues is the case. It was therefore impossible for us to give theoretical formulæ such as is given in Professor Hartley's paper, which would explain all the change taking place in these muds; and, consequently, we adopted one which would give a general, and so far explanatory, view of what takes place. We were aware that the changes we endeavoured to point out in our paper were believed by some authorities to be simply the results of the ingestion or digestion of sulphates by bacteria, namely, bacterium hydro-sulfureum ponticum, and bacterium sulfureum, which are supposed to be able to effect this change; but at the time we brought this subject before the Society (1891) the then known facts in connection with these organisms, and the power they had ascribed to them, were of too vague and unsatisfactory a nature to be relied upon, and we chose rather to explain the facts resulting from our experiments in a manner as simple as was possible with known chemical data. Professor Hartley, in this connection, refers to Andrussow's paper, read before the British Association at Edinburgh in 1892, "On the Condition of the Water in the Black Sea under the 100 Fathoms Line"; but, so far as we know from personal communication with Andrussow and the meagre statement which appeared in the Royal Geographical Society's Magazine, we incline to doubt that living organisms can exist in water which is practically a saturated solution of sulphuretted hydrogen; but on this point we have no experiments of our own to bring forward.

A reference is made to the "bacterium hydro-sulfureum

ponticum" by the Franklands in their book on "*Micro-Organisms in Water*," published in 1894, at page 458. It is not impossible that these organisms can live in such extraordinary conditions as described. However, the authors (Zelinsky and others) do not inform us whether the organisms were living or dead when found in the Black Sea mud. If living, this would prove the fact claimed for it, that this bacterium is able to live under such circumstances; but if dead, it simply represents an organism which had been poisoned by its own effete products, and whose remains have sunk to the bottom and accumulated with the other organic remains there. In our paper we refer to this condition of the Black Sea, first brought into notice by Andrussow and others, but we accounted for the presence of sulphuretted hydrogen in such an amount there owing to the sea being practically land-locked, having no oceanic circulation, and where the amount of iron present in the suspended mud brought down by rivers was not sufficient to absorb or combine with the sulphur of the deoxidised sulphates, and so form a blue or black mud such as is deposited on ocean floors where ferruginous clays are abundant. The ferric oxide in these muds is readily blackened by sulphuretted hydrogen.

Professor Hartley takes exception to the statement we made, that when sulphuretted hydrogen in solution in pure water is left exposed to air for a sufficient length of time, part of the sulphur combined with hydrogen as sulphuretted hydrogen, is oxidised into sulphuric acid. Our experiments were made with such care that we cannot accept as conclusive the argument produced to the contrary. As we performed our experiments, it was immaterial to us whether the sulphuretted hydrogen was decomposed or oxidised with the deposition of sulphur or not; if so, the sulphur being in a fine state of division, in presence of water may have oxidised into sulphuric acid. The point that we wished to bring prominently forward was, that when a solution of sulphuretted hydrogen is exposed to the influence of air or oxygen, changes take place which, in the end, produce sulphuric acid. As we have shown from our experiments, sulphuretted hydrogen, when in excess, decomposes calcium and magnesium carbonates, both always present in blue muds, producing sulphides of these metals; and it is beyond question that the sulphides so formed are, in their turn, decomposed

by excess of carbonic acid present in these muds, sulphuretted hydrogen being set free.

With regard to the fermentation of Cellulose and its products, we leave Professor Hartley's conclusions as they stand, having no criticism to offer, as the subject has not been especially studied by us; and we would, in conclusion, remark, that the chemical changes occurring and recurring in such an exceedingly complex compound as is represented by oceanic blue mud, cannot be written down even by the most complicated reactions or formulæ, but which, on the other hand, might be a simple matter if the organic remains present in these muds were only cellulose, as is represented in Professor Hartley's paper. A principal object of our paper on the blue muds was to explain how vast chemical reactions were continually taking place between the sea-water salts and ferruginous muds or clays and decomposing organic matter, one of which was the abstraction from the sea and storing up of sulphur in a stable condition in muds which, in time, become rocks, containing that element generally in the form of sulphide of iron.

Later knowledge compels us to admit that the changes in these fermenting blue muds may be wholly dependent on the influences of bacteria.

Note of a Case of Early Appreciation of Musical Pitch.

By **John G. M'Kendrick**, M.D., Professor of Physiology in the University of Glasgow.

(Read February 17, 1896.)

A boy has recently come under my observation in whom the appreciation of pitch is developed at so early an age and with so remarkable a degree of accuracy as to justify a record being made of the case.

His name is John Baptist Toner. He was born on 11th June 1891, so that he is now a little more than four and a half years of age. He is a fine healthy-looking boy. His parents, who are young, are both musical. The mother sings and has a keen appreciation of music. The father plays on both the piano and the organ, has all his life taken much interest in music, and has studied the theory of the art. So far as can be discovered, neither the grandparents, nor any member of collateral branches of the family, were distinguished by musical ability.

Since he was two years of age, the boy has had access to a piano, and he seems to find pleasure in fingering the keys. During the last week of 1895, his father first taught him the names of the notes on the piano, and he says that his little boy picked up this information with astonishing rapidity. He acquired the names of the white keys in two or three minutes, in his first lesson, and the names of the black keys were acquired on the following day in an equally short period of time. Since that date, he has not forgotten the names of the notes, and when any note is sounded, by striking a key on the piano, he invariably can tell the name of the note, simply after hearing the sound, and without seeing the key struck. Not only so, but he can name two notes when two notes are struck on any part of the keyboard, and he can name the notes of any chord, or give the names of the notes when any three keys are struck on the piano at the same moment.

I examined the boy in the first instance in his father's house.

He was at the other side of the room from where the piano stood, with his back towards me, and he bent his head over a sofa. Keys were then struck at random all over the keyboard, and he made no mistake in at once giving the name of the note. Thus, adopting the notation of Helmholtz* for the different octaves, he gave e'; f''; $a'\flat$; $f'\sharp$; $f'\natural$; e'''; $G_{''}$; $e'\flat$; $c\sharp$; a'; $a'\flat$; d'; d''; $f'''\flat$; $f'\natural$; $F_{''}\sharp$; $G_{'''}$. I then tested him by double sounds, and he gave correctly $e'\ g'$; $f''\ g'$; $d'\ c'$; $c'\ g'$; $g\ c'$; $b'\ a'$; $c'\sharp\ f\sharp$; $b\flat\ c'\sharp$; $b\flat\ f'\sharp$; $c'\sharp\ b'\flat$; $e'\flat\ f'\sharp$; $G_{,}\ f''\sharp$; $c'\ b''$; $c_{,}\ d'$; $b'\flat\ a\flat'$. He then resolved, without error, the following triple sounds:—$E_{,}\ c'\ g''$; $b'\ g'\ D_{,}$; $g'\ e'\ G$; $c''\sharp\ e'\ a_{''}$; $c'\sharp\ e'\flat\ A_{''}\flat$; $f'\ d'\ g'$; $d''\ a'\ F_{''}\sharp$. These are only examples of the trials to which he was subjected. The piano was as nearly as possible at concert pitch.

On another occasion, I tested his ear with a set of Koenig's forks in which the middle c of the piano = 256, lower than concert pitch. He thus listened to tones lower in pitch than those to which he was accustomed. In each trial, after a very short time for consideration, he gave a name to the tone which nearly corresponded to the pitch with which he was familiar. Thus c' (Ut = 256) was called d, and so on, except when I sounded a fork giving concert pitch, when it was at once named correctly. Knowing nothing about what is meant by sharpening and flattening a note, but attaching a value or meaning to the sounds produced by striking the black keys of the piano, the boy named the sound as it corresponded to his standard. Thus he at once recognised c' (concert pitch), and although the note was flattened, it still, to him, was c', till it came to $c\flat$ (a name, however, which was not in the child's nomenclature) and then it became b, and was always answered as b until b was actually reached. In cases where the child knew the name of the semitone, such as $g\sharp$, then the boundary to him was a quarter of a tone (half of a semitone).

This child has therefore acquired a standard of pitch, and he remains faithful to this standard under all circumstances. Thus he can sing, so far as the compass of his little voice will allow, any note one may name, and he invariably sings it correctly at concert pitch. His ear always keeps him right to his own

* The middle c of the pianoforte is c', or Ut_3, in French notation.

standard, or, in other words, he has a gift of absolute perception of pitch in conformity with his standard.

After this trial, he came to my house and was tested by a Bechstein piano and an American organ, both at concert pitch. He made very few mistakes, and it was only reasonable to attribute these to the distraction caused by the new surroundings in which he was placed. When I caught his attention he was invariably correct.

Those who practise the tonic sol-fa system are expected to sing any note of the scale when the tonic is given, and, no doubt, tens of thousands of children can do this without difficulty. The test for the elementary certificate of the Tonic Sol-fa College is as follows:—

> "Pitch the key note by means of a given C, sol-fa not more than three times, and afterwards sing to words or to the syllable *laa* any 'part' in a Psalm or Hymn tune in the tonic sol-fa notation, not seen before, but not necessarily containing any passages of transition, or of the minor mode, or any division of time less than a full pulse."

This quotation is given to show that the tests are quite different from those which were applied to this child, who had no key or reference note given to him to aid him in his search after particular notes. Here, however, is a case where pitch is appreciated directly, and where no musical education has been imparted. It is true that in one sense there has been a musical education of the ear, as during the last two years the boy has touched every key on the piano and listened to the sound; but it is only about a month since he acquired the names of the notes. He has also listened almost daily to good music. He now associates the names a, b, c, g, etc., with particular sounds within the whole range of the piano.

I observed that in resolving a triplet of notes he always mentioned first the one highest in pitch, and he seemed to "feel about," as it were, for the other notes, but the correct answer was given within a quarter of a minute. After a good many trials his attention flagged, and then, occasionally, he made mistakes. In the fingering with his own piano, the only error committed was with the lowest note of the instrument, and this note was out of tune. It was also curious to notice that the boy was astonished that his

father had any difficulty in naming notes, when he, the boy, struck them on the piano. He was impatient and even irritable when one inadvertently mistook the answer he gave. Thus, if he named f♯ and I said g, he emphatically shouted: "No, no, f♯, f♯"!

This case is of importance in connection with the question of tonal fusion and the analytic powers we undoubtedly possess. A person gifted with a "good ear" may, by long practice, acquire a power something like that possessed at so early an age by this boy; but it is well known, on the other hand, that many skilful musicians are deficient in this faculty, and that they fail to develop it by practice. One can hardly resist the conclusion that it is a gift dependent on the delicacy of the ear and the part of the brain that receives auditory impressions, and that each note of a given instrument has, to such an individual, an undefinable quality or colour by which it is identified.*

* See an article on *Amusic* (*musikalische aphasie*) by J. G. Edgren (Stockholm), in *Deutsche Zeitschrift für Nervenheilkunde*, 1895. The following quotation is of interest: "Bei einer grossen Anzahl von Kindern entstehen die Tonbilder vor den Wortbildern, und viele singen, ehe sie sprechen. Bei einigen organisiren sich die Tonbilder mit erstaunlicher Leichtigkeit. Reyer berichtet über ein 9 Monate altes Kind, das die auf dem Klavier angeschlagenen Noten genau wiederholte. Stumpf's Kind sang die scala exact im Alter von 14 Monaten. Der Sohn eines componisten, Dvořák aus Prag, sang, als er ein Jahr alt war, den Fantinitzamarsch mit seiner Amme. In Alter von 1½ Jahr sang er die Melodien seines Vaters, welche dieser auf dem Klavier begleitete."

Demonstration of the Acoustic Turbine or Sound Mill of Alfred M. Mayer* and V. Dvořák.† By John G. M'Kendrick, M.D., Professor of Physiology in the University of Glasgow.

(Read February 17, 1896.)

Dr M'Kendrick showed this apparatus as made by Dr R. Koenig. It consists of four small and light cylindrical resonators, made of aluminium, tuned to $Ut_4 = 512$ vibs. p. sec., attached to the ends of four light bars forming a cross. The cross, carrying the resonators, is balanced on a fine pivot placed vertically. When the fork is sounded (by an electro-magnet between the limbs) the cross carrying the four resonators begins to rotate. The motion arises from the greater pressure at the node at the closed end in the interior of the resonator nearest the source of sound than on the outer surface of this end. If the mouth of the cylinder is directed towards the source of sound, the resonator is repelled by the greater pressure within. The next resonator is slightly attracted, but it is brought within the sphere of greater pressure mainly by the inertia of the first resonator, and so on.‡

* Invented by A. M. Mayer in January 1876, and shown on May 22nd, 1876, to the New York Academy of Sciences.

† Invented independently by V. Dvořák, and described by him in *Annalen der Physik und Chemie*, Band III., No. 3, dated 10th November 1877.

‡ See also *American Journal of Science*, third series, vol. xvi. p. 22.

Note on a Sensitive Flame. By John G. M'Kendrick, M.D., Professor of Physiology in the University of Glasgow.

(Read February 17, 1896.)

Dr M'Kendrick showed an extremely sensitive flame made by Dr R. Koenig. It is constructed on the type of the sensitive flame apparatus devised by Barrett* and improved by Govi. A Lecomte † burner directs the gas towards a fine wire gauze screen, and when the gas is lit above the screen the flame is very sensitive at ordinary pressures. The special part of the apparatus now shown consisted of a funnel or resonator placed at right angles to the Lecomte gas-jet and opening into the space below the gauze screen. To ensure success a fine stop-cock or clip must be used for the regulation of the quantity of gas. Dr M'Kendrick showed also that by placing a glass tube 460 mm. in length by 40 mm. in diameter over the flame, and about 4 mm. above the level of the gauze, as first suggested by Geyer,‡ a musical note was heard which was disturbed by vibrations acting on the flame. The apparatus not only showed the ticking of a watch, placed in the resonator, by the movements of the flame, but with each "tic" the tube sounded slightly. The slightest movement in the vicinity of the apparatus agitated the flame; and as the latter was arranged so as to be near the point when it evoked the tone of the tube, now and again the tube gave forth a strong tone as a vibration acted on the mass of gas below the gauze.

* Barrett, *Phil. Mag.*, 1867.

† Lecomte, *Phil. Mag.*, 1856.

‡ Described at p. 65 of *Sound*, by Alfred M. Mayer. London, 1879.

Note on Mr Alfred Graham's Method of producing Sound by an Electrical Arrangement. By John G. M'Kendrick, M.D., Professor of Physiology in the University of Glasgow.

(Read February 17, 1896.)

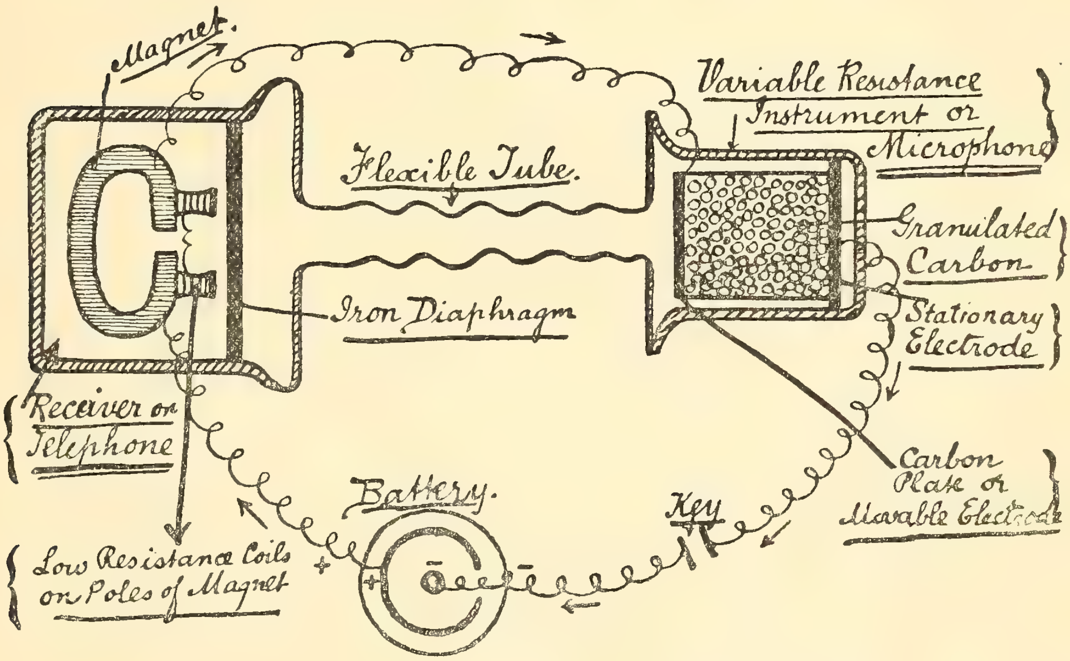
During the past year I have had many opportunities, in connection with a research on the phonograph, of observing the efficacy of Mr Graham's method of electrically producing sound, and as it is not generally known, a short explanation may be useful.

Two or more cells are connected to a mass of granular carbon whose resistance can be varied by vibration, and the circuit is then completed through the coils of an electro-magnet. In front of the electro-magnet is a diaphragm sensitive to the variations of current flowing through the electro-magnet. This diaphragm is so placed relatively to the circuit of variable resistance, that any vibration imparted to it is transmitted through a column of air to the circuit of the variable resistance or its diaphragm. The granular carbon, similar to that in telephonic transmitters, is contained in a box or chamber closed at one end by a diaphragm of carbon, with which the granular carbon is in electrical contact. The carbon resistance box is so placed that its diaphragm is acted on by the vibrations set up by the diaphragm of the electro-magnet. To effect this, a flexible tube passes from the front of the diaphragm of the transmitter to the front of the diaphragm of the electro-magnet. Thus, when the various parts of the apparatus are connected, as seen in the figure, and when the circuit is completed by closing a key, the vibrations set up in the telephone diaphragm are caused to act upon the circuit of variable resistance, which, in turn, again acts upon the telephone or receiver diaphragm. A continuous musical sound is then emitted.

The receiving or telephonic element of the apparatus consists of a powerful permanent magnet carrying upon its poles spools wound with low resistance coils. This acts on an iron diaphragm of considerable area, about 90 mm. in diameter, and of minimum

thickness, so that a maximum amplitude of vibration may be obtained. The element of variable resistance is composed of a stationary electrode of carbon mounted in a case of insulating material. A diaphragm of carbon is fixed in the case. This forms the movable electrode of the apparatus, and between the stationary and movable electrodes is placed a quantity of carbon granules.

When at rest, and before the key completing the circuit is closed, the iron diaphragm is held in tension by the permanent magnet. On closing the key, the coils of the electro-magnet energise and the



Plan of Graham's Electrical Method of Producing Sound.

diaphragm is attracted. This movement of the diaphragm operates through the air column in the flexible tube and the carbon diaphragm is acted upon, thus increasing or diminishing the resistance of the circuit of variable resistance. Thus, for example, increase of the resistance decreases the strength of the battery current and causes a return movement of the receiver diaphragm, which, through the air column, produces a return movement of the diaphragm of variable resistance, causing a decrease in the resistance of the circuit. The consequent increase of the battery current, in turn, increases the energy of the receiver coils. Thus reciprocal effects are produced, resulting in the production of a musical

sound, which lasts as long as the circuit is kept closed. The pitch, loudness, or quality of the sound may be modified by varying the strength of the battery current, by changing its direction, or by varying suddenly the length of the column of air, or the pressure, in the flexible tube (see diagram).

Measurements made for me by Mr Alexander Galt, of the Physical Laboratory of the University of Glasgow, showed the resistance of the receiver (the electro-magnet) to be 2 ohms, while that of the variable resistance fluctuated between 13 and 29 ohms. The apparatus works well with two of Obach's dry cells, Q pattern, which, with the resistance just mentioned, give about one-fifth of an ampère.

The apparatus is interesting, not only on theoretical grounds, but because it suggests the possibility of constructing a new kind of musical instrument. Thus diaphragms might be tuned to the notes of the scale, and by pressing on keys, and thus completing circuits, musical notes having something of the quality of those of brass instruments might be produced. Possibly, also, by piercing holes at proper distances in the flexible tube, these holes might be so fingered as to produce different sounds, and thus we might have an electric flute. I have found a modification of Mr Graham's method of great service in connection with the phonograph. It might also be adapted to the purpose of rendering audible in a room the sounds of respiration, or even the sounds of the heart.

The Cranial Nerves of *Chimæra monstrosa*. By **Frank J. Cole**, Demonstrator of Zoology, University College, Liverpool. *Communicated by* Professor Ewart, F.R.S. (*Preliminary Communication.*)

(Read March 2, 1896.)

This work was commenced at the suggestion of Prof. Ewart, F.R.S., to whose kindness I am indebted for the material used, and for much assistance and advice during the progress of the investigation. Prof. Ewart placed two specimens at my disposal—a male and a female, both of which were about 43 cm. long, excluding, of course, the lash. As, however, the male had been partly dissected for special points in connection with another investigation, it was only available for the study of the IXth and Xth cranial nerves; the main bulk of the present work, therefore, having been carried out on a single specimen. Hence there are several points on which my work will require confirmation.

As the dissection of the cranial nerves of *Chimæra* elucidated facts of a more interesting character than I had anticipated, it was thought desirable to publish a preliminary statement containing a brief account of the facts, and to leave the full description with figures and morphological deductions for a future communication, which I hope to lay before the Royal Society in a few weeks' time. The present paper, therefore, pretends to be a brief review of the more important facts and nothing more.

Having nothing new to describe with respect to the olfactory and optic nerves, I shall commence with the oculo-motor:—

Third Nerve.—This is a large nerve, and arises from the crus cerebri by two principal roots just behind the pituitary body. On emerging from its foramen in the orbit, it immediately gives off a branch to the superior rectus muscle of the eye, and divides into two large dorsal and ventral branches running respectively over and under the optic nerve. The dorsal branch courses straight across the orbit and fans out on the internal rectus muscle, whilst the ventral, after giving off a branch to the inferior rectus, pursues a somewhat similar course and supplies the inferior oblique. The ventral division of the IIIrd, just after its origin, gives off a fine

nerve, which, being joined by a similar nerve from the profundus division of the Vth, swells to form the ciliary ganglion, and this, on being teased, was seen to contain large nerve cells. From the ciliary ganglion two ciliary nerves with the characteristic wavy course proceed to the eye. This account of the ciliary ganglion agrees with that described in *Lœmargus* by Ewart, but I should like to leave the matter in *Chimæra* an open one, as I only dissected the ciliary ganglion on one side, and then, owing to the parts having been disturbed by previous dissection, not as carefully as I could have wished.

Fourth Nerve.—Arises, as in all vertebrates, from the roof of the Sylvian aqueduct, and emerges from the brain in the perpendicular furrow between the optic lobes and cerebellum. It courses upwards and forwards, and reaches the orbit under cover of the superficial ophthalmic division of the VIIth. It is obscured by the latter for the greater part of its course, but finally takes a dorsal curve and supplies the superior oblique muscle of the eye.

Fifth Nerve.—This nerve is in a more primitive condition than in any vertebrate yet described, and its distribution, therefore, is somewhat important. It arises from the medulla by two closely applied roots under cover of the buccal root of the VIIth. These two roots fuse indissolubly, and neither they nor their product mingle with the roots of the VIIth, or in any way become inseparable from them. The Vth nerve, therefore, may be followed throughout almost the whole of its course without the confusing elements which mixed roots inevitably introduce.* The Vth, after passing through the same foramen which gives exit to the buccal and hyomandibular divisions of the VIIth, immediately expands into the large Gasserian ganglion, and I distinguish five branches as follows:—

(*a.*) Superficial ophthalmic. } Dorsal branches.
(*b.*) Profundus. }
(*c.*) Maxillary (præ-branchial).
(*d.*) Mandibular (post-branchial).
(*e.*) Pharyngeal or Visceral.

* There may be an exchange of very fine fibres between the roots of Vth and VIIth, and the outer buccal division of the VIIth I have seen connected with the maxillary division of the Vth, but these are obviously questions of detail.

(*a.*) *Superficial Ophthalmic.*—This nerve is exceedingly interesting, since it does not fuse with the nerve of the same name from the VIIth, and its distribution may therefore be ascertained without any of the doubt necessarily attached to its distribution in the Elasmobranch fishes. It arises from the Gasserian ganglion and courses straight upwards, and after crossing the superficial ophthalmic of the VIIth, runs straight forwards to be distributed to the skin, over and in front of the orbital region. We may, therefore, conclude with certainty that this branch of the Vth does not in *Chimæra*, and doubtless also in other fishes, innervate any of the sense organs of the lateral line. As an important variation, I may note that I have seen it send a few fibres to the superficial ophthalmic of the VIIth as it passes over it.

(*b.*) *Profundus.*—This branch of the Vth is also of great interest. In the first place, it is undoubtedly a branch of the Vth, and does not arise by a separate root from the medulla. It springs from the main trunk of the Vth, slightly distal to the Gasserian ganglion, and after giving off a twig to the ciliary ganglion, runs straight forwards across the orbit dorsal to the optic nerve. It perforates the cartilage of the cranium at the anterior end of the orbit, and courses upwards to fan over and completely fuse with the superficial ophthalmic branch of the VIIth. Its distribution after this point is necessarily a difficult, if not an impossible, question to determine. When the profundus has traversed about two-thirds of the orbit, it gives off a conspicuous branch (which may immediately divide into two), and this branch, running in a canal bored in the cartilage of the cranium, passes over the superficial ophthalmic of the VIIth, and eventually reaches the skin in front of the orbit. Here it gives off two important twigs, which *innervate two sense organs of the supra-orbital canal* (marked black in the figure).* This was found in both specimens. The Vth nerve, therefore, has two dorsal sensory branches—one to the skin, and the other to the skin and sense organs of the lateral line.

(*c. and d.*) *Maxillary and Mandibular.*—These nerves will be fully described in my future paper, but do not possess features of sufficient interest to be mentioned here.

* This figure was kindly drawn for me by a senior student of the college, Mr E. J. W. Harvey.

(*e.*) *Pharyngeal or Visceral.*—This is a branch of the maxillary division of the Vth, and arises ventrally from one of the main branches of the maxillary, dipping down and running backwards, and ultimately passing on to the pharynx at the point where the lower jaw articulates with the skull. It is a slender nerve with a long course, and was not traced far on to the pharynx, having by this time become extremely fine.*

The Vth, then, in *Chimæra*, is a perfectly typical cranial nerve, in so far as it possesses dorsal sensory branches to the skin and sense organs, a pharyngeal branch to the mouth, and finally pre- and post-branchial branches (maxillary and mandibular).

Sixth Nerve.—Arises by six rootlets from the ventral pyramids of the medulla at a level slightly behind the dorsal root of the VIIth. It runs forwards and slightly downwards, and reaching the orbit by a foramen under cover of the roots of Vth and VIIth, passes straight into the external rectus muscle of the eye, which it supplies.

Seventh Nerve.—This arises from the medulla by three main roots, two of which are ventral and the other dorsal. The dorsal root (which arises just behind the restiform bodies, and on a level with their dorsal border) is connected mostly with the buccal, but also with both the other roots by distinct branches. At no point, however, is there any confusion between the roots of the VIIth and Vth. The seven divisions to be distinguished in the skate and *Læmargus* exist also in *Chimæra*. These are :—

(*a.*) *Superficial Ophthalmic.*—Has dorsal and ventral roots, and courses over the eye to be distributed to the supra-orbital canal (marked by cross-hatching in the figure—*SO*), and the superficial ophthalmic group of ampullæ, which with its fellow of the opposite side occupies by far the greater part of the cavity of the snout. As this nerve passes through the canal in the cranium to reach the orbit it swells into a large ganglion.

(*b.*) *Buccal.*—Constitutes the main part of the dorsal root of the VIIth, and reaches the orbit through a foramen which also gives exit to the remainder of the VIIth and the Vth. In passing through the cranium it expands into the large buccal ganglion.

* I might describe other visceral branches of the Vth, but shall postpone doing so until I have dissected other specimens.

The buccal nerve, which accompanies the main trunk of the Vth for some little distance in the orbit, but is easily separable from it, divides into inner and outer buccal trunks, and innervates the sensory canals lettered B, B^1, and B^2 in the figure (marked by dots), and terminating in inner and outer buccal groups of ampullæ. The latter group differs from the others previously described in that the ampullæ are much smaller, being, in fact, only about half the size of those in the superficial ophthalmic and inner buccal groups.

The hyomandibular and facial proper arise from the medulla by a common ventral root, which, however, receives dorsal fibres from

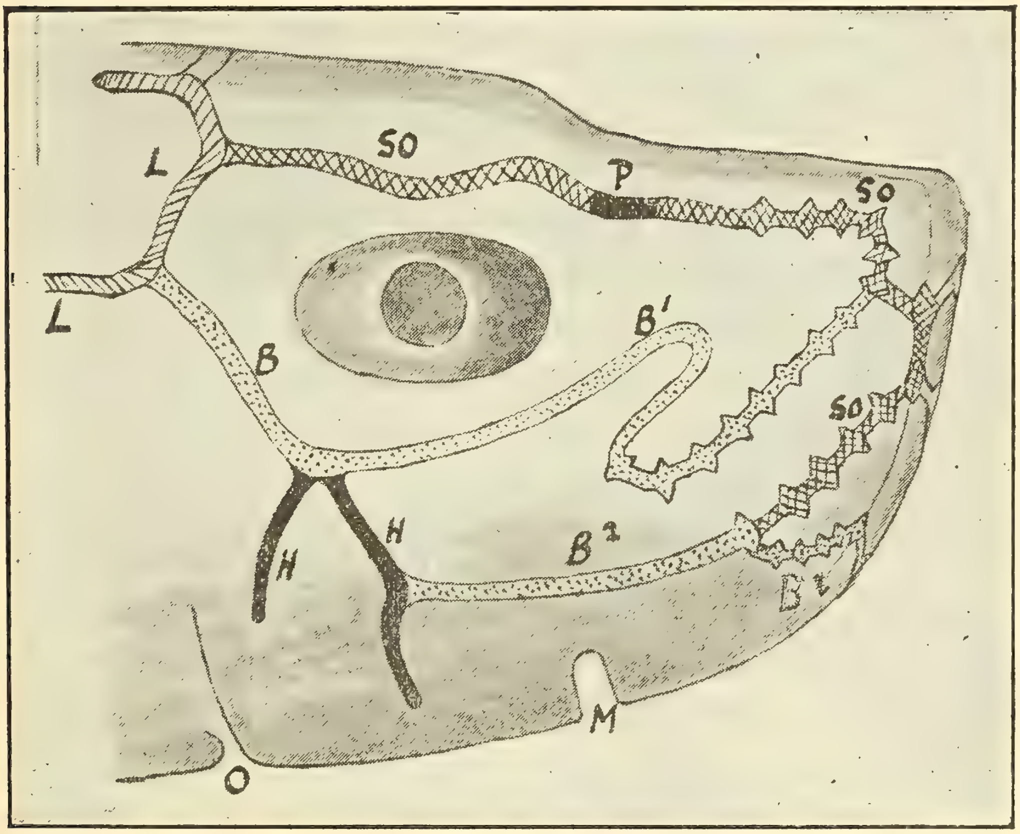


Diagram to show innervation of the sensory canals (indicated by different kinds of shading).

SO, supra-orbital canal (cross-hatched); *B*, B^1, and B^2, infra-orbital canal (dotted), supplied by the main trunk, and the inner and outer buccal nerves respectively; *P*, profundus portion of supra-orbital canal (black); *H*, hyomandibular canal (black); *L*, lateralis canal (oblique shading); *M*, mouth; *O*, opercular fold.

the root of the buccal; whilst in the cranium it expands into the large hyomandibular ganglion, and immediately after gives off palatine and chorda tympani branches, the root of the former con-

taining numerous ganglion cells, thus constituting the ganglion of the facial proper. I did not, however, in the common root, distinguish between the hyomandibular and facial proper divisions.

(*c.*) *Hyomandibular.*—Runs downwards and slightly forwards, finally terminating in a very few ampullæ and a peculiar gelatinous tissue under the skin over the first branchial cleft. Whether the practical absence of ampullæ and the presence of this peculiar tissue point to the disappearance of the hyomandibular group of ampullæ in connection with the disappearance of the spiracular cleft, is a question which must be left for future investigation to determine. The hyomandibular supplies the posterior of the hyomandibular canals (marked black in the figure—*H*) and sends a few twigs to the anterior.

(*d.*) *External Mandibular.*—This nerve is a branch of the hyomandibular (?) and runs downwards and forwards in front of the other branches of the hyomandibular to supply the greater part of the anterior division of the hyomandibular canal, and two groups of ampullæ situated behind the lower jaw—one fairly large and the other very small, though perfectly distinct and innervated by a separate nerve. These ampullæ are peculiar, being small and simple. Whilst, therefore, the outer buccal ampullæ differ from the superficial ophthalmic in size only, the external mandibular ampullæ differ from them not only in size but in structure also.

(*e.*) *Facial Proper.*—Although there is no spiracle in *Chimæra*, this, the motor division of the VIIth, is well represented by two large nerves. Of these, one supplies the superficial muscles of and in front of the opercular fold, whilst the other, after giving off a largish branch which anastomoses with the chorda tympani, supplies the ventral portions of the same muscles, and also sends a deep branch to the muscles of the hyoid arch. These two nerves may correspond with the præ- and post-branchial divisions of the facial proper in the skate and shark.

(*f.*) *Palatine.*—This large nerve, as already mentioned, springs from the fused hyomandibular and facial proper trunk just distal to the hyomandibular ganglion, and has numerous ganglion cells at its base, which probably identify the ganglion of the facial proper. It runs forwards and downwards, and is distributed mainly to the

roof of the mouth, and to the mouth in the immediate neighbourhood of the teeth of the upper jaw.

(*g*.) *Chorda tympani.*—This is a conspicuous nerve arising just distal to the palatine, and is distributed to the sides and floor of the mouth. It receives a branch from the facial proper as described above.

The facial, then, is to a great extent a typical cranial nerve. It has four dorsal sensory branches (superficial ophthalmic, buccal, external mandibular, and hyomandibular), two visceral branches (palatine and chorda), and præ- and post-branchial branches (?).

Eighth Nerve.—Arises from the side of the medulla by a single root situated in the angle formed by the root of the buccal and the ventral root of the superficial ophthalmic of the VIIth. It immediately expands into a large ganglion, and gives off the usual ampullary and vestibular branches. The vestibule contains a *hard calcareous* otolith of a very curious shape.

Ninth Nerve.—As figured by Hubrecht * in his memoir on the skull of the *Holocephali*, this nerve emerges from the cranium by a separate foramen. It arises from the medulla by a main root and two rootlets under cover of the root of the lateralis, and expands into an obvious ganglion immediately on emerging from the cranium. From the ganglion a very fine dorsal branch is sent up to the skin, but this does not innervate any sense organs of the lateral line. There are three pharyngeal branches, but none of these correspond to the pharyngeals of the vagus in their precise distribution. There are præ- and post-branchial branches to the first and second demi-branchs respectively, on the hyoid and first branchial arch, and both of these are continued ventrally on to the pharynx, thus forming two accessory pharyngeal branches, or five in all. Two nerves are also given off which accompany and supply the first branchial arch.

Tenth Nerve.—The vagus in *Chimæra* is in a specially interesting condition, and is more primitive than in any other vertebrate. Without any dissection beyond mere exposure, four ganglia are to be easily distinguished, and were it not that the ganglion of the third branchial lies under, and is obscured by that of the second,

* *Niederländ. Archiv f. Zool.*, Bd. 3. Except that the foramina for the Vth and VIIth are confused, Hubrecht's figure is perfectly accurate.

the vagus of *Chimæra* would be in the unique condition of consisting of five perfectly distinct nerves. As far as I am aware, this condition is most nearly approached in *Torpedo*, the IXth and Xth cranial nerves of which I dissected some time ago at Professor Ewart's suggestion. In *Chimæra* the vagus arises by five main roots, of which the most anterior is the root of the lateralis. The next three roots are those of the three branchial nerves which supply the posterior demi-branch (or third) on the first branchial arch, two demi-branchs on both the second and third arches, and a single demi-branch on the anterior face of the last or fourth branchial arch. There are thus five gills, of which the first and last (on the hyoid and fourth branchial arches) consist of a single demi-branch only, and the remaining three of two demi-branchs each. The distribution of each branchial nerve to a great extent resembles that of the IXth, and will be described in my next paper. The posterior root of the Xth is that of the intestinal. This nerve forms an elaborate plexus on the stomach and œsophagus, and the only nerve from the vagus to the heart which I have yet found, is a very fine branch of the intestinal which ran in the wall of the sinus venosus. In several Elasmobranchs I have found a nerve from the last branchial division of the vagus going to the heart, but have hitherto not seen any traces of it in *Chimæra*. The lateralis, besides innervating the lateralis canal, gives off a nerve dorsally to the commissural and median canal (shown by oblique shading—*L*). This dorsal branch requires very careful dissection, as it would be very easy to mistake its origin, as, indeed, I did mistake it myself until a careful revision showed me my error. Further complications in dissecting the vagus are the dorsal branches from the anterior spinal nerves passing through the cranium, which seem to arise from the IXth and Xth. I shall describe these nerves fully and the other spinal nerves in my future paper.

Conclusion.—The study of the cranial nerves of *Chimæra* undoubtedly emphasises the affinities which the Holocephali have with the Elasmobranchs, and more particularly with the sharks, but it may be added with equal certainty that both as regards the anatomy of the brain and the distribution of the cranial nerves, the Holocephali are more archaic and have not undergone such differentiation as the Elasmobranchs evidently have.

Experiment illustrating the Modern Theory of Salt-Solution. By Professor Crum Brown.

(Read January 20, 1896.)

It is well known to all chemists that sulphuretted hydrogen does not give a precipitate of zinc sulphide in a solution of zinc chloride or zinc sulphate if a sufficient (quite small) quantity of a strong acid, such as hydrochloric or sulphuric acid, has been added to the solution. It is also quite well known that if we add a sufficient quantity of a solution of sodium acetate to the clear solution containing the zinc salt sulphuretted hydrogen and strong acid, we throw down the whole of the zinc as sulphide. This used to be explained as follows:—Hydrochloric acid acts on zinc sulphide thus, $ZnS + 2HCl = H_2S + ZnCl_2$, and therefore the opposite action $ZnCl_2 + H_2S = ZnS + 2HCl$ cannot take place in the presence of hydrochloric acid. But as acetic acid is too weak an acid to act on zinc sulphide, the presence of acetic acid does not prevent the precipitation of zinc sulphide, and the hydrochloric acid originally there, as well as that produced by the reaction, acts on the sodium acetate to form sodium chloride and acetic acid. On the modern theory the explanation is this:—The zinc sulphide is attacked not by the hydrochloric acid, but by the hydrogen ions; these are present in the solution of hydrochloric acid; and that acid being to a great extent "ionised," we have a great concentration of hydrogen ions. There is a limiting value for the concentration of hydrogen ions, above which the action $ZnS + 2H^{\bullet} = Zn^{\bullet\bullet} + H_2S$ takes place, so that zinc sulphide cannot be formed by the action of sulphuretted hydrogen on a zinc salt in a solution containing hydrogen ions with a concentration above this limiting value. The addition to a solution containing hydrogen ions of a salt of a weak acid, such as acetic acid, diminishes the concentration of the hydrogen ions. Sodium acetate is ionised to a very much greater extent than hydrogen acetate, and therefore when we add sodium acetate we are adding not only $Na\bar{A}$ but also $Na^{\bullet} + \bar{A}'$, and some of the $\bar{A}'$ ions so introduced unite with $H^{\bullet}$ ions to form $H\bar{A}$, and so diminish the

concentration of $H^{\cdot}$ ions. We can thus diminish this concentration below the limiting value above referred to, and thus allow of the precipitation of the whole of the zinc as ZnS.

Either of these explanations is sufficient to account for the phenomena. But we can arrange a crucial experiment to decide between the two theories. To a solution of ferrous acetate add enough acetic acid to prevent the precipitation of FeS on addition of sulphuretted hydrogen; add sulphuretted hydrogen to this acid solution of ferrous acetate, of course no black precipitate appears; now add solution of sodium acetate, and FeS is thrown down. As there is here no acid present but acetic acid, the first theory offers no explanation of the phenomena, but the second does. We can add to ferrous acetate enough acetic acid to have the concentration of $H^{\cdot}$ ions above the limiting value for FeS (which is much below that for ZnS), and so prevent the formation of FeS; and we can add to this solution containing ferrous acetate, acetic acid, and sulphuretted hydrogen, enough sodium acetate to diminish the concentration of the $H^{\cdot}$ ions below the limiting value, and so allow of the formation of FeS.

Preliminary Note on the Structure and Affinities of Phoronis. By Arthur T. Masterman, B.A. (Cant.), Assistant Professor and Lecturer in Zoology at the University of St Andrews.

(Read March 16, 1896.)

In spite of the great amount of attention which has been bestowed upon this group by many workers, it must still be said that our knowledge of its systematic position is very meagre and uncertain, and that there are points in its anatomy which require elucidation. Through the kindness of Professor M'Intosh I have been enabled to examine some specimens of *P. australis*, and also a *Phoronis*, which appears to be a new species. I reserve a detailed account of my results for later publication, and here only refer very briefly to leading points. I may mention that Professor M'Intosh has also allowed me the inspection of his serial sections of *P. buskii. M'Intosh*, and after a careful comparison I have not the slightest doubt that this is a distinct species from *P. australis*—it has been usual to regard the distinction between these two forms as not of specific value.

Divisions of the Body.—It is usual, in describing the structure of *Phoronis*, to refer to two different parts of the body which lie before and behind the septum respectively. I propose, for reasons shown later, to emphasise the division of the body into—(1) The epistome, lying dorsally to the mouth, and having very definite, though somewhat involved, relationship to the next part; (2) the tentacular region, which I prefer to call the collar, consisting of a ring round the mouth region, the oral part being produced into two arms or processes which bear tentacles and are coiled, and the aboral end being limited superficially by the nerve ring and fundamentally by the so-called septum; (3) the trunk, including all the region behind the septum.

These three divisions are not mere conveniences of description, but are definite segments, having body-cavities separated from each other by mesenteries, and differing in form and function amongst themselves.

The tentacles, as is well known, are arranged upon the lophophore

in two parallel rows, an outer and an inner, the inner being on the concave side of the coiled lophophore and the outer on the convex.

Between these two rows of tentacles is contained an imperfectly shut-off cavity, which we may term the "branchial space." Its roof is open at the apex of the tentacles, its lateral walls are formed by the tentacles, and its floor is formed partly by the epistome, and, partly on the outer side of it, the mouth. The inner row of tentacles is in close contact with the epistome throughout this cavity, except in the middle line. Here the tentacles are discontinuous for a small space, those of each side, however, overlapping so that a "branchial fissure" is produced which leads from the branchial space into the lophophoral concavity.

The lophophoral arms are coiled round so that the inner row of tentacles also tend to inclose a space which we may term the "atrial space." This opens freely above, has the outer wall of the collar below, and is closed in by the inner row of tentacles, except in the median line. Here it opens orally to the branchial fissure and aborally to the exterior.

Into the atrial space are discharged the waste products from the anus, nephridia (including sexual products), and lophophoral organs.

Both branchial and atrial spaces in the large species of *Phoronis* are continued up the three coils of the arms.

Ectoderm.—The ectoderm consists, for the most part, of three types of one-layered epithelium.

The trunk has numerous circular transverse furrows which are found to be due to a plication of the epithelium. This is a permanent and normal folding of the glandular cuticle-secreting cells, and is not, as formerly asserted, due to contraction during the processes of killing and preserving.

In the collar and epistomial regions there occur the two other types of epithelium. The first or branchial has very much elongated glandular ciliated cells, with small nuclei; they are found covering the whole surface of the branchial space, as defined above, and are continuous with others of a similar type lining the œsophagus. These cells secrete copious mucus, strings of which are noticed in section to be passing down the tentacles and into the mouth, carrying numerous food particles adhering to them. This method of food ingestion forcibly recalls that of the Tunicata. The epistome pro-

jects over the branchial space at an angle, and evidently serves to sheer off the water current and direct it through the branchial fissure into the atrial space.

The epistome is continued as a ridge projecting across the floor of the branchial chamber the whole way round the coils, which ascend in a spiral, so that it forms a beautifully constructed apparatus for separating the water and food currents.

The third type is the atrial epithelium, the cells of which are small, compact, and cubical, with large nuclei; they usually have numerous pigment spots, and are mostly non-ciliated. This type lines the whole atrial space except the lophophoral organs and the nerve ring, and the branchial fissure, so that the upper surface of the epistome is (in the median line) covered with this epithelium, the lower surface with the branchial type.

Nervous System.—This is ectodermal, and has been described by

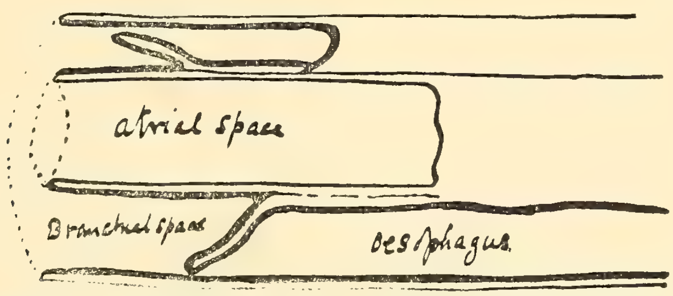


Diagram of Longitudinal Section through Phoronis to one side of median line.

various workers, who do not, however, agree entirely. Cori gives a detailed account of the nerves in *P. psammophila*, with which I find,

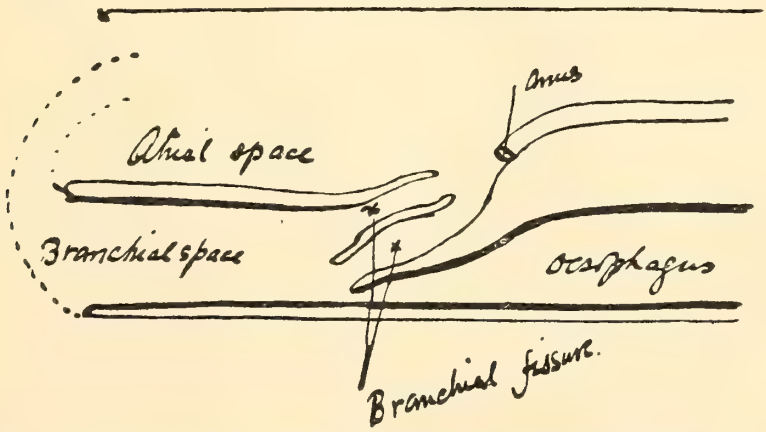


Diagram of Longitudinal Section through Phoronis through median line. In each diagram the branchial epithelium is thick-, the atrial thin- lined.

for the most part, *P. australis* agrees, except that, as Benham stated, there are *two* longitudinal nerves (paired) near the lateral mesentery.

I fail, however, to understand the latter's statement that the nervous system is "mainly ventral." All the main nervous concentrations lie in the atrial floor; in fact, in or close to the line joining mouth and anus.

Lophophoral Organs.—These appear to be compound organs, consisting of (1) the ciliated lophophoral nerve ring; (2) excretory cells in a mass at the base of atrium; (3) ectodermal flap or funnel facing inwards, and in contact with the inner row of tentacles. This latter consists of ciliated ectoderm inside and atrial epithelium outside; between these is a continuation of the collar body cavity. At the base of the funnel is a minute aperture on each side opening into the collar cavity. I believe the flap to be the remnant of a dorsal lamella, and the pores to be true collar pores. The lophophoral organ or flap extends for some way (quite a quarter of a coil in *P. australis*), and is not short, as has been figured by one observer.

Body Cavities.—Each of the divisions of the body already referred to, namely, epistome, collar, and trunk, has a well-defined cœlomic cavity, separated from each other by mesenteries.

Between the cœlomic peritoneum and the ectoderm is the "basement tissue," which is apparently the result of the secretory activity of cells which lie imbedded here and there in the matrix, and which are probably mesoblastic in origin (Caldwell). This skeletal structure does not appear to differ in any essential degree from cartilage, and may be directly compared to the "chondroid tissue" of *Balanoglossus*. It lines the whole body under the ectoderm and between the cœlomic walls. Thus, between the collar and trunk cavities, it forms a thick "septum" or "diaphragm," to which are attached the longitudinal muscles. This chondroid tissue and the vascular spaces evidently occupy the blastocœlic cavity. The tissue is not present to any extent in the very young *Phoronis*.

The mesentery between the epistome and the collar supports the lophophoral and tentacular veins, but is not thickened by chondroid tissue.

On either side of the branchial fissure each collar cavity has a short diverticulum which ends in a minute aperture to the exterior, the collar pore. The epistome and its cavity is produced into two long processes which run dorsally to the collar cavity in each arm. It has a communication by a small pore on each side into the

collar cavity. All the cavities tend to be obliterated by mesenchymatous tissue.

Trunk Cavities.—The longitudinal muscles run from septum to base, and are inclosed in special parts of the cavity, shut off by thin walls, from the general cavity of the cœlome. The trunk cavity is bent upon itself, but this reduplication does not affect the collar or epistome regions.

As regards the vascular system, the dorsal vessel runs in the space between the gut and the body cavities, and the ventral vessel evidently belongs to the space in the ventral mesentery, but is secondarily deflected over to the left cœlomic cavity.

Coming to the affinities of *Phoronis*, I am led to formulate the view that this genus must take its place amongst the *Hemichordata*, in spite of the obvious fact that in none of the known species have a notochord or gill-slits been described.

In his report upon *Phoronis buskii*, Professor M'Intosh was led to compare several of the organs, such as the nervous system, with those of *Cephalodiscus*, and he even suggested comparisons with *Balanoglossus*. Mr Harmer, in his Appendix to the Challenger Report upon '*Cephalodiscus*,' also suggested some points of resemblance between this form and *Phoronis;* and Professor Lankester, in his article in the *Encyclopædia Britannica* upon '*Polyzoa*,' pointed out features in common between the Pterobrachia and the Vermiformia. We can accept this observer's comparison between the forms here mentioned without following him in his constitution of the *Polyzoa* or the *Podaxonia*.

The formation of the group *Hemichordata*, involving the removal of *Cephalodiscus* and *Rhabdopleura* into phyletic connection with *Balanoglossus*, tended, to those who accepted this classification, to disguise what I believe to be the true affinities of Phoronis; especially because the presence of a notochord and gill-slits were stated to be essential characters of the group. This is not the place to go fully into the views held with regard to the affinities of *Phoronis*; various workers have allied it to the *Gephyrea*, the *Brachiopoda*, the *Chætopoda*, or even the *Polyzoa*.

I have embodied my main points of comparison of *Phoronis* with the *Hemichordata* in a table, all details being left over for the present. We may here deal with the two chief points in

which Phoronis differs from the others, namely, the absence of a notochord and gill-slits.

We may first take into consideration the notochord.

This, though an important organ in *Balanoglossus*, is greatly reduced in *Cephalodiscus* and in *Rhabdopleura*. In all three it is intimately connected with the proboscis (syn. epistome or pre-oral lobe), and shares its degree of prominence with this organ.

In the case of *Phoronis*, we already know from its ontogeny that its larval form, *Actinotrocha*, has a very well-developed pre-oral lobe, but that this atrophies almost entirely, leaving only a vestigial epistome (Caldwell). This being so, we must seek for the notochord, not in the adult Phoronis, but in the *Actinotrocha* just before its metamorphosis.

To prophesy is a dangerous pursuit, especially in morphology, but I cannot help suspecting that subsequent investigations will reveal some trace of an organ homologous to the notochord in the *Actinotrocha* stage.

Such a discovery, although convincing, is not, however, essential to our contention, for a notochord may never have been evolved in *Phoronis*, and yet its alliance with the *Hemichordata* be unassailable.

Secondly, there are no gill-slits. This is a feature which, so far as is known, is not found at all in one of the *Hemichordata*, namely, *Rhabdopleura*, and the gill-slits differ in number in structure, and even, perhaps, in function in the other two. Apart from this, we have pointed out that the "branchial fissure" in *Phoronis* performs the function of the gill-slits of *Cephalodiscus*; and although, of course, not morphologically equivalent, yet its presence points out a factor which might easily account for the non-evolution or atrophy of true gill-slits in the gut region.

We may assume that the reduction of the epistome, consequent upon a sedentary life, enabled the outer row of tentacles to be carried across the median line ventrally, and thus formed the branchial space above referred to, which physiologically can be compared in every detail with the pharynx of *Tunicata*: the same comparison holds true for the branchial fissure and the atrial space. *Phoronis* having therefore, through loss of function by the pre-oral lobe, been enabled to evolve a complete branchial and food-collecting area (by

the approximation and fusion of post-oral tentacles), all situated in front of the *true mouth,* we at least should not expect to find organs fulfilling similar functions in the endodermal area behind the mouth.

Although, therefore, there are no notochord nor gill-slits in the adult *Phoronis,* yet we can find analogous organs and therein—reasons for the forestalling or replacement of these two structures.

A few words with reference to the Table of Comparison : It will be seen that *Phoronis* offers various points of resemblance to each usually acknowledged member of the group, and thereby lends support to the constitution of the group as such.

We may note that, in sedentary habit, pre-oral lobe, and collar-region, *Phoronis* most nearly agrees with *Cephalodiscus* and *Rhabdopleura* ; and in the shape and structure of the trunk, including vascular system, it more nearly resembles *Balanoglossus.*

In comparing these two forms, I would amplify Wilson's suggestion ; and by separating the anal half of the trunk from the anterior half, in fact, straightening out the trunk, would eliminate a point of dissimilarity which is due to a secondary adaptation on the part of the tubicolous *Phoronis.* (*N.B.*—It is well to note that a similar mode of comparison must be held valid by those who contend for a phyletic connection between *Balanoglossus* and the other *Hemichordata.*)

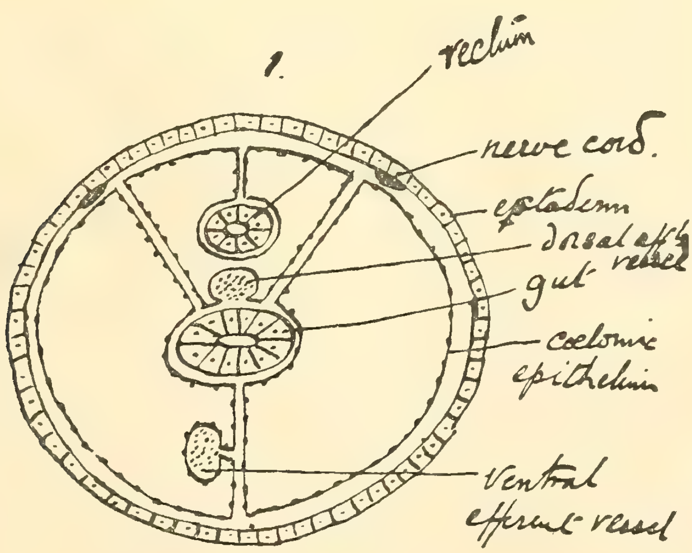


Diagram 1 may be taken to represent a transverse section through the trunk region of *Phoronis,* the rectum being detached from its secondary connection with the lateral mesentery, and the ventral

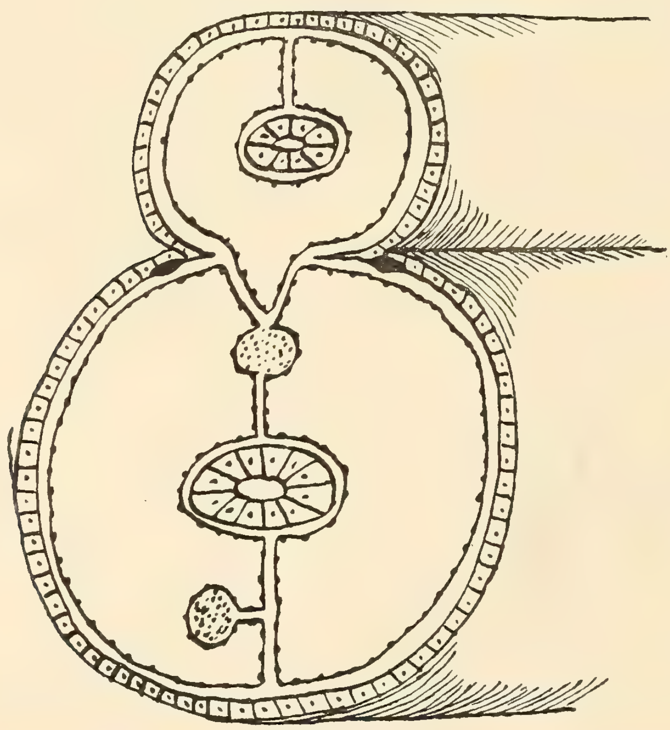
efferent vessel being partially moved back from its true asymmetrical position.

These two asymmetrical features are probably connected the one with the other. I leave out the nephridia in order not to complicate matters.

In the region of the stomach the two blood-vessels communicate round the gut by a sinus formed between it and the cœlomic epithelium.

In diagram 2 the separation is partially effected so that the lateral mesenteries are seen to be composed of the dorsal mesentery

2.



of the anterior half of the trunk and the dorsal peritoneum of the posterior half.

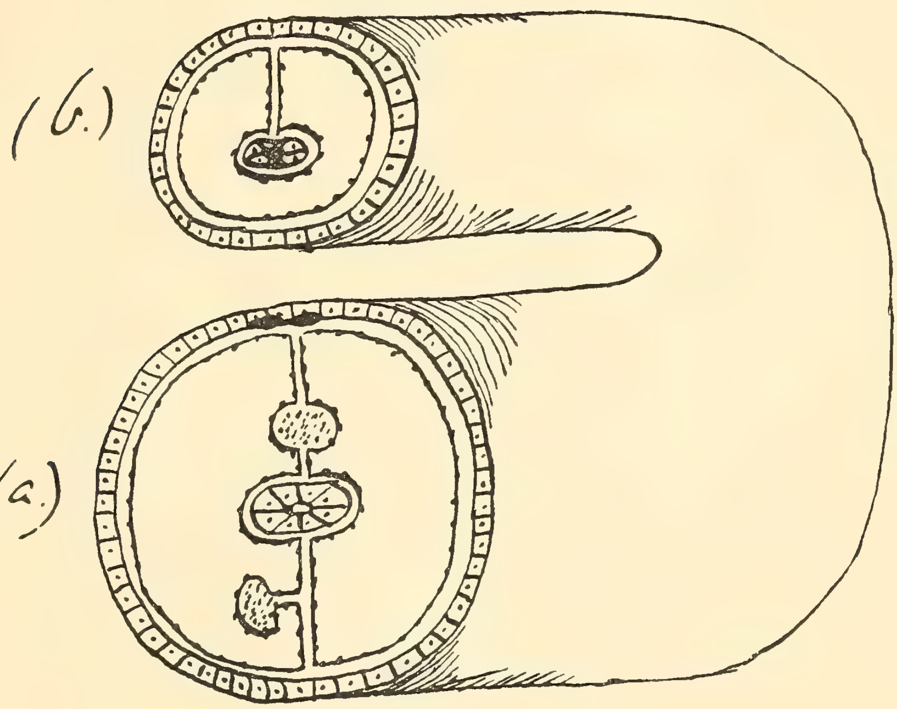
As the posterior part of the trunk moves out it allows of the approximation of the two anterior elements of the lateral mesentery to approach one another in the median line, and the same applies to the two nerve cords.

In diagram 3 is depicted the effect produced by a complete separation of the two trunk halves. (*a*) Is a transverse section through the anterior half, and (*b*) an inverted transverse section of the posterior half.

This section (*a*) I would compare directly to one of *Balanoglossus.*

The main features are :—Simple epidermis, with mucous glands and nerve fibrils, concentrated dorsally in the median line; a dorsal and ventral mesentery, the former incomplete in the hind region, as in some species of *Balanoglossus*; dorsal afferent blood-vessel in dorsal mesentery, communicating with the ventral efferent vessel by enteric sinus; ventral efferent vessel connected with ventral mesentery, and dividing into two in collar; cœlomic pouches, largely filled with connective-tissue, sexual glands, a thin layer

3.



of circular muscles, and within them a layer of longitudinal muscles, inserted anteriorly in chondroid tissue, posteriorly in body wall; cuticular secretion of ectoderm, forming more or less permanent "tube."

I would define the *Hemichordata* as follows :—

Cœlomate animals, with three parts of the body, an impaired pre-oral and two paired post-oral, each opens typically to the exterior by ciliated pores; ectoderm simple, with mucous glands, partially or wholly ciliated; nervous system ectodermal, concentrated in dorsal collar region, at base of epistome, and in post-oral collar ring—there may or may not be dorsal or ventral cords or both; simple black or coloured pigment in the ectoderm cells—a cuticular secretion of the

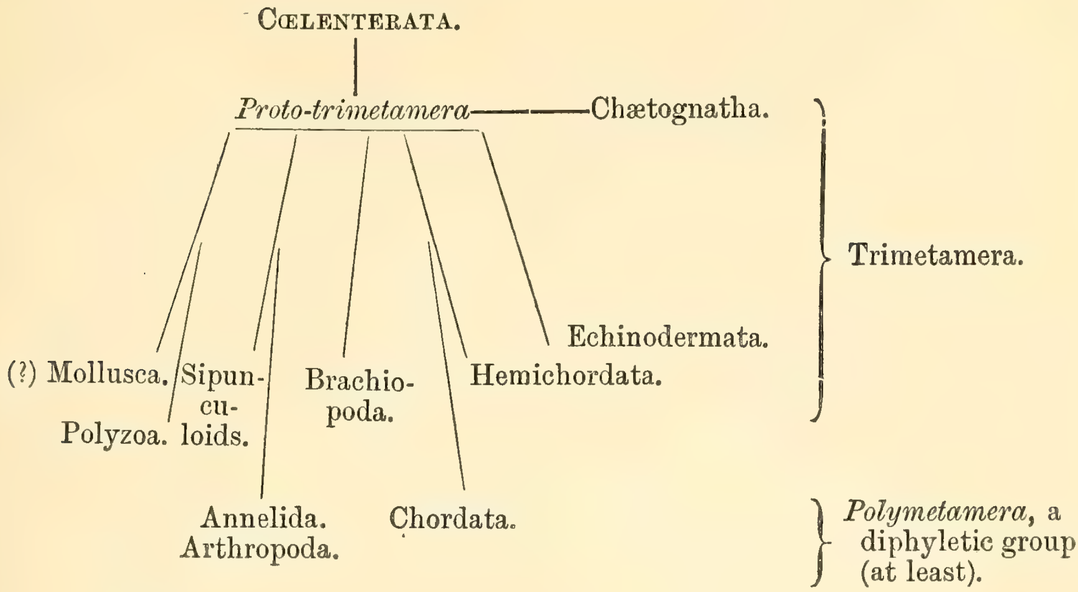
ectoderm forms a more or less permanent and connected skeletal structure; endoderm simple and glandular, mostly ciliated—that in dorsal collar region forms in most a vacuolated skeletal area, the notochord; and in some are paired endodermal openings to the exterior in the trunk region, gill-slits; there are five cœlomic pouches usually secondarily filled with connective-tissue, and the trunk cavities containing the generative organs; there may or may not be a vascular system and well-differentiated muscular layers; a mesodermal chondroid tissue, hypertrophied at various parts. Diœcious or hermaphrodite, solitary, sedentary, colonial, or gregarious.

It is undeniable that *Phoronis* shows close affinities with the *Brachiopoda* and the *Gephyrea*; and, on the other hand, *Balanoglossus* shows equally remarkable affinities with *Echinodermata.* I believe these resemblances will all eventually find expression in the constitution of one large primitive group which we may provisionally term the *Trimetamera*, emphasising their fundamental feature of having the mesoderm divided up into a pre-oral and two post-oral cœlomic pouches, all, primitively, opening to the exterior by ciliated pores, the earliest condition of nephro-gonaducts. The pre-oral segment is specialised for locomotion and for sense functions, and degenerates upon the assumption of a sedentary life. The 1st post-oral segment is mainly connected with locomotion and ingestion of food, and is usually tentacular, whilst the trunk segment, or 3rd post-oral, is concerned with digestion and reproduction.

From this type, by segmentation of the trunk, are derived the polymetamerous forms (*Chætopoda*, *Annelida*, and *Chordata*).

The *Trimetamera* I believe to be the most primitive *Cœlomata* derived directly from the *Cœlenterata*, and they persist in varied forms to the present day in the *Hemichordata*, *Echinodermata*, *Brachiopoda*, *Chætognatha*, unarmed *Gephyrea*, and possibly the *Mollusca.*

The division of the body into *three* segments has been made permanent in these forms, because of a peculiar physiological and morphological fitness in this number, as seen by a secondary acquirement by polymetamerous forms of a prosoma, mesosoma, and metasoma, or head, thorax, and abdomen, which can be exactly compared (physiologically) to the three segments of the *Trimetamera.* The following table will express the likely relationships:—



I cannot here enter into the ontological resemblances between *Phoronis* and the other *Chordata*, general resemblances will at once suggest themselves.

We may summarise the steps by which *Phoronis* has departed from the general type of *Hemichordata* as follows :—

1. Assumption of sedentary life, with degeneration of pre-oral lobe, carrying with it the atrophy of notochord and proboscis pore.
2. A synchronous tentacular hypertrophy of collar region.
3. A synchronous reduplication of trunk region.
4. Further reduction of epistome, with growth of tentacles across mid-ventral line to form branchial space.
5. Fusion of base of tentacles, and dorsal coiling of lophophoral arms, fusing at their bases with body, and thus forming atrial space.
6. Atrophy of ventral half of collar flap, dorsal half persists as dorsal lamella.
7. Atrophy of gill-slits (if ever present) in correlation to the perfection of extra-oral branchial apparatus.
8. Further hypertrophy in large species of *Phoronis*, of branchial system by further coiling of epistome and collar arms.

	Balanoglossus.	Phoronis.	Cephalodiscus.	Rhabdopleura.
Divisions of Body.	Proboscis. Collar 2. Trunk 2.	Epistome. Collar (fused). Trunk 2.	Buccal shield. Collar 2. Trunk 2.	Epistome. Collar 2. Trunk 2.
Shape of Body.	Elongated and not reduplicated.	Reduplicated but elongated.	Reduplicated.	Reduplicated.
Shape of Collar.	Cylindrical. An operculum.	Two arms bearing tentacles in two rows. Lophophoral lamella.	Twelve arms bearing tentacles in two rows. Post-oral lamella	Two arms bearing tentacles in two rows. Post-oral lamella.
Shape of Præ-oral Lobe.	Large and cylindrical.	Small and lamellar.	Large and cuspidate.	Small and cuspidate.
Apertures.	One or two proboscis pores. Two collar pores. Genital ducts.	No proboscis pores. Two collar pores. Two genital nephridia.	Two buccal pores. Two collar pores. Two genital ducts.	No proboscis pores. Two collar pores. ?
Ectoderm.	Unicellular epidermis. Ciliated and mucous cells. A tubular skeletal structure. (In some.)	Unicellular epidermis. Ciliated (partly) and mucous cells. A tubular skeletal structure. (In most.)	Unicellular epidermis. Ciliated (partly) and mucous cells. A branching skeletal structure.	Unicellular epidermis. Ciliated (partly) and mucous cells. A tubular branching skeletal structure.
Nervous System.	At base of ectoderm. Concd. in collar (ring), base of epistome, dorsal and ventral cords. No ciliated sense organs.	At base of ectoderm. Concd. in Collar (ring), base of epistome, dorsal cords. Paired ciliated sense organs at base of collar-arms.	At base of ectoderm. Concd. at collar (ring), base of epistome. ...	As in *Cephalodiscus.* Paired Ciliated sense organs at base of collar-arms.
Pigment.	Pigment ectodermal. Many colours.	Pigment ectodermal. Many colours.	Pigment ectodermal. Many colours.	?
Mesoderm. Body Cavities. (*See* Divisions of Body.)	Cœlomic cavities tend to fill up with connective tissue. Trunk has genital organs. Dorsal (incomplete in some) and ventral mesenteries in collar and trunk. Vascular vessels dorsal, ventral, and lateral in collar. Longitudinal and circular muscles in trunk inserted in chondroid tissue. Collar cavities in operculum.	Cœlomic cavities tend to fill with connective tissue. Trunk has genital organs. Dorsal (incomplete) and ventral mesenteries in trunk. Vascular vessels dorsal, ventral, and lateral in collar, produced into tentacles. Longitudinal and circular muscles in trunk inserted in chondroid tissue. Collar cavities produced into tentacles and lamella.	Cœlomic cavities tend to fill with connective tissue. Trunk has genital organs. Dorsal and ventral me enteries in trunk. None. Longitudinal and circular muscles only in stolon. Collar cavities produced into tentacles and lamella.	 None. None. Collar cavities produced into tentacles and lamella (?).

	Balanoglossus.	Phoronis.	Cephalodiscus.	Rhabdopleura.
Sexes.	Unisexual.	Hermaphrodite.	?	?
Mesoblastic Skeleton of Chondroid Tissue.	Below notochord and in collar.	Well - developed in epistome, in collar and in tentacles.	Well - developed in buccal shield, and in tentacles.	Well - developed in tentacles.
Endoderm.	Notochord.	... ?	Notochord.	Notochord.
Gill-slits.	Many.	None.	One pair.	None.
Gut.	Pharynx. Oesophagus. Stomach. Intestine.	Pharynx. Oesophagus. Stomach. Intestine.	Pharynx. Oesophagus. Stomach. Intestine.	Parynx. Oesophagus. Stomach. Intestine. ?
Anus.	Slightly dorsal.	Dorsal.	Dorsal.	Dorsal.
Habit.	Solitary.	Gregarious.	Co-habiting.	Colonial.
Habitat.	Burrowing.	Sedentary and burrowing.	Partially sedentary.	Sedentary.

Some Points in the Physiological Chemistry and Coagulation of Milk. By **David Fraser Harris**, B.Sc. (Lond.), M.B.C.M., F.R.S.E. *Communicated by* JOHN G. M'KENDRICK, M.D., Professor of Physiology in the University of Glasgow.*

(Read March 2, 1896.)

I. THE PHYSICO-CHEMICAL CONDITION OF CASEINOGEN IN MILK.

According to the latest views of physiological chemists, the innumerable fine particles of milk are identified with the caseinogen. For instance, Halliburton† regards these particles as caseinogen and nuclein which are obviously not fat-globules; and Stirling‡ unhesitatingly accepts the view of the particulate nature of this proteid, adducing as proof the fact of the absence of all particles from the filtrate of milk through a porous clay-filter.

I used a "Berkefeld" diatomaceous filter (holding 150 c.c.) said to "sterilise" water. On immersing it in milk, and by creating a partial vacuum inside it, there was obtained an exhaustion-filtrate perfectly limpid and free from all particles. Unless sterilised by heat, a vigorous growth of fungi appeared in it, there being abundant nourishment for them, seeing that it contains lactose, chlorides, phosphates, sulphates, calcium, and a proteid (undoubtedly the lact-albumin), but is incapable of clotting with rennet. Its reaction is faintly acid. Caseinogen cannot, then, be held to be "dissolved" in the plasma, in the ordinary sense of a "solution," else this pressure-filtrate would be coagulable; and only as a last resource should we fall back upon the rather unsatisfactory hypothesis that it may be present as molecules too large to pass through the pores of a diatomaceous filter.

There is in milk undoubtedly, besides what we have been accustomed to call the oil-globules, a very large number of extremely minute particles or granules exhibiting Brownian movement, and

* From the Physiological Laboratory of the University of Glasgow.

† In his *Chemical Physiology and Pathology*, 1891, p. 574.

‡ In his *Outlines of Practical Physiology*, 1895, p. 96.

it is these that are held to be caseinogen exclusively, while the globules are said to be only fat. Optically, however, all these particles behave alike; the smallest refract light exactly as do the largest, but when one set are focussed the others are out of focus, so that they do not all look bright or dark at once. Further, all these particles appear to stain with 1 per cent. osmic acid.

Neither globules nor granules could be stained with any of the following:—Carmine, picrocarmine, hæmatoxylin, and eosin, and I have completely failed to demonstrate the presence of an envelope or membrane round the globules, much less to prove the assertion that it is of equal thickness round particles of all sizes, as is asserted by Oliver.*

We are warranted in asserting that the union of fat with caseinogen is an exceedingly intimate one, whether the proteid be a membrane or be interstitially associated with the fat, and may, I think, say that whereas the globules consist of a maximum of fat and a minimum of caseinogen, the granules have a minimum of fat and a maximum of caseinogen. It is familiar to chemists that if we try to precipitate the caseinogen we precipitate the fat, and if we try to clot the caseinogen we entangle the fat; in short, whatever happens to the caseinogen happens to the fat, and *vice versâ.*

With the microscope I scrutinised a large number of clots, precipitates, and wheys to find a striking similarity in all. The acetic acid-precipitate in milk consists of masses of the very smallest particles agglutinated together, many of the globules being present along with them—the so-called "precipitate of caseinogen entangling the fat." There is not an appearance of fibre. But cream itself, "the fat," *par excellence*, is, when treated with acid, microscopically the same thing, save that the relative proportion of globules to granules is reversed.

A morsel of the clot (produced by rennet) is, under the microscope, extremely like the acid-precipitate—a multitude of fine granules cohering with great tenacity, with relatively few globules caught amongst them. There is no vestige of fibrillation. The precipitate by saturated sulphate of magnesium is, again, an aggluti-

* Late Principal of the Western Dairy Institute, Berkeley, in his work *On Milk, Cheese, and Butter*, p. 34.

nation of a large number of the granules. All these particles, whether in clot or precipitate, stain with osmic acid.

Further, the so-called "pure solutions of caseinogen" or "suspensions" (which seems to be much nearer the truth), whether in water or in dilute $MgSO_4$, have as many globules and granules as whey or diluted milk, *e.g.*, milk diluted to one in four. Apart from these particles they have no caseinogen, and the exhaustion-filtrate of "pure caseinogen" will not only not clot, but contains no proteid or chemical substance at all; it is water.

The whey from the acid-precipitate in milk, or the whey from the rennet clot, are alike turbid, owing to the large number of both globules and granules which they contain; accordingly, I find I can obtain from either of them a precipitate with acetic acid, and a clot with rennet, calcic phosphate, and phosphoric acid present together, of course very open as solidifications, yet, microscopically, indistinguishable from the original clots in milk, and with all their particles stainable by osmic acid.

Further, as scrutinised under the microscope, the actions of acid and rennet on a hot-stage are identical, resulting in an agglutination of multitudes of granules and some globules, many of both kinds remaining isolated in the whey.

Halliburton* states that Hoppe-Seyler got as much "casein from cream, as from portions of milk below the cream"—presumably from equal weights of cream and milk below it. In other words, there is as much caseinogen associated with the large oil-globules (*the cream*) as there is with the smaller globules and smallest particles in the skimmed milk below.

Cream is but the comparatively quickly-formed aggregation of the larger oil-globules risen to the surface, these being so much lighter than their bulk of plasma; a centrifuge hastens this separation, but never effects a complete separation of the "cream" or "fat," for the same reason that after a certain time some particles will never rise to the surface of skimmed milk, it can be no more "creamed"; but is still opalescent, and contains fat on analysis.

Now, if these smallest particles were fat alone, they ought in time to rise to the surface of skimmed milk, and ought always to be separated by the "centrifuge"; the only inference is, not that

* *Chemical Physiology*, p. 575.

they are pure caseinogen, but that they are fat-particles weighted down or loaded by caseinogen till they are of exactly the same specific gravity as milk-plasma, from which, therefore, they never rise, nor can be separated by centrifugalisation.

If they were pure caseinogen, skimmed milk should contain no fat; but it does contain fat; its particles stain with osmic acid like full milk. In any case, if any of these particles were pure or "naked" fat, there would be nothing to prevent their coalescing, the milk would not be the emulsion it is. Thus skimmed milk gives very little *fat* compared with cream (into which all the large globules have risen), because all the very smallest particles only are left in it, but it gives as much *casein* as cream, because associated with these myriads of specks of oil is some not inconsiderable burden of proteid, enough to raise their specific gravity from that of oil to that of milk plasma.

The envelope-theory appears to have several things to recommend it:—

(1.) The permanent emulsion, which milk is.

(2.) The analogy from artificial emulsions, where egg-albumin or gum is made the coating of oil-globules.

(3.) Cohesiveness of these oil particles in precipitates and clots, amounting in certain cases, *e.g.*, in old shrunken clots, to extreme tenacity; naked oil-globules could not exhibit this property.

(4.) The action of an alkali in apparently dissolving off something, so that the fat can then be dissolved by ether, &c. Even without ether the globules can now coalesce into large drops, which have lost the *spherical* appearance and dark periphery of fat-globules.

(5.) The fact that something is broken or ruptured in "churning" milk, whereby the oil-globules accumulate and caseinogen is apparently liberated; for butter-milk contains a larger percentage of casein than milk skimmed. This is just what we would expect, for instead of fat being taken out of it as cream with its associated caseinogen, all the fat is left in it, but disintegrated there, and afterwards removed, having set free in the plasma its associated proteid, which, being the caseinogen of the cream, comes to be added in analysis to the casein of what would otherwise have been skimmed milk whose percentage of casein is thus considerably raised.

Hence, I think, we are justified in only saying at present that

both the largest and the smallest particles in milk contain fat in direct ratio to their bulk, and caseinogen in indirect ratio to the same; that the caseinogen is associated with the fat in a most intimate fashion, yet one which can be destroyed by a mechanical process such as "churning."

II. On Caseinogen—its Chemistry and Coagulation.

Following Halliburton, the term casein is reserved for the chief proteid of milk when it has been rendered visible as a clot or precipitate by the action of an enzyme, usually rennin of rennet, sometimes bacterial ferments.

Acetic acid is said merely to precipitate the caseinogen; precipitation also is the effect of saturating milk with $MgSO_4$.

I studied the chemistry of "pure caseinogen" with Ringer's solution or suspension, made by grinding up the washed acetic acid precipitate with fine prepared chalk and throwing the mixture into a couple of litres of distilled water. The chalk particles fall to the bottom, some of the large globules rise to the top, while the water in between contains the smaller globules and granules. We thus obtain an odourless, opalescent, neutral liquid of varying specific gravity (depending on the amount of water used) which, unless sterilised by boiling, rapidly becomes putrid and clotted. Sterilised and well corked, it keeps for a fortnight or more. It boils at 103° C., and like milk, especially in an open dish, it skins over; this pellicle can be nothing else than a surface dessication of "caseinogen."

As in milk this proteid is entirely precipitated by saturating with $MgSO_4$, a chemical and not a mechanical process, inasmuch as such precipitation entirely fails when ground glass, fine sand, dust, or chalk is agitated with the liquid. The various familiar proteid precipitants, as might be expected, entirely precipitate caseinogen both pure and in milk; corrosive sublimate does not appear to precipitate it in milk. The corrosive mineral acids (in minim doses) precipitate caseinogen both in milk and when "pure," excess of the acid in many cases effecting a partial solution. Acetic acid of only 10 per cent. tends in excess to dissolve its precipitate. Picric acid does not precipitate it in milk, but does in pure solu-

tion. Tartaric and citric acids precipitate it in milk, but not in pure solution, not even when their powders are boiled with it. Carbolic acid fails to precipitate it in either condition. Lactic acid when dilute precipitates it in pure solution, but not in milk when either dilute or strong. When strong it does not precipitate it even in pure solution. This inertness of lactic acid is very remarkable when compared with its alleged activity as a precipitant produced by fermentation from lactose.

On the other hand, tannic, benzoic, oxalic, salicylic, and phosphoric acids precipitate caseinogen in both conditions.

Boracic acid, so far from precipitating it, is a very efficient antiseptic; mixed with it milk appears not to become putrid at the end of three months.

Milk alone would not, therefore, seem to be an antidote for picric, carbolic, or boracic acid poisoning.

With an alkali, *e.g.*, KHO, NaHO, or $NH_4.HO$, milk, especially if heated, becomes less opalescent, and if further heated some precipitate may appear. In a very short time the solution becomes yellowish-brown from decomposition and some saponification of the fats. Upon neutralisation or acidification an abundant precipitate falls. With certain acids, the neutralisation precipitate is dissolved in excess of the acid, caseinogen in these circumstances behaving like a true alkali-albumin.

Pure caseinogen treated, *e.g.*, with $NH_4.HO$, is "dissolved," in that the solution is clarified; neutralisation or acidification precipitates it, and excess of weak acid only slightly dissolves it.

The Conditions affecting the Coagulation of Caseinogen.

Various preparations of rennet differ in several respects from one another. I used two "essences":—(1) Duncan, Flockhart & Co.'s, a highly acid, glycerine extract of the gastric mucosa of the calf. It is not quite lime-free. In details of experiments this is designated "A.R." (acid rennet). (2) Martindale's "pure essence." This is an NaCl-extract, containing, therefore, chloride of sodium. It is by no means lime-free either. It is the "M.R." of my notes. In some experiments I neutralised it with $NH_4.HO$: this is "N.R."

After having added rennet to a solution containing caseinogen and kept the mixture for sometime (30 minutes) at 40° C., the non-appearance of a clot is not now-a-days held to prove that caseinogen has not been acted on by rennet, but merely that the casein has not been precipitated.

I have corroborated the value of Ringer's test of the formation of this soluble casein, which, for brevity, I shall call *pro-casein*, based on his belief in the two-fold nature of the process of clotting, viz.:—

(1.) *Stage one.*—The conversion of caseinogen by rennet into a soluble form of casein (gerinnung).

(2.) *Stage two.*—The precipitation or solidification of pro-casein into a more or less cohesive clot or curd—casein—in presence of or by the action of certain substances (abscheidung). This is shown by *boiling* the solution containing the rennet, so as to kill the enzyme and prevent its further action. When cold, some precipitant of pro-casein is added to the tube, usually $CaCl_2$, and if after heating, a precipitate now falls, it is assumed that the rennet did its work independently of the $CaCl_2$ or other precipitant. One cannot demonstrate stage two in ordinary milk, because clotting occurs in it without the addition of any precipitant; it can be well shown in diluted milk or in pure solutions of caseinogen; thus, dilute milk (1 in 4) heated at 40° C. with M.R. gave no clot, it was then *boiled*, and on cooling 4 m. of $CaCl_2$ (10 per cent. solution) were added, when a clot appeared. Had the solution not been boiled, we should have called it a case of clotting due to the adjuvancy of $CaCl_2$; we now know the rennet acted without any assistance from the $CaCl_2$, which merely precipitated the preformed pro-casein. ($CaCl_2$ precipitates pro-casein with great rapidity, and, except in dilute solutions, as a firm curd, whereas it precipitates caseinogen in milk very much more slowly, at a much higher temperature, and often only as a slimy deposit). The importance of a free acid in effecting stage two is well brought out by the following experiments. Ringer's pure caseinogen or milk + N.R., heated gave no clot; boil each solution, and add 1 in acetic acid to each, when a clot forms in each. Had we not previously boiled we might have imagined the rennet had not been able to act until the acid was added, whereas what really occurred was that the rennet did its work in the neutral medium, the result of that work

becoming a visible effect only in the acid medium. Both $CaCl_2$ and acetic acid (10 per cent.) are precipitants of pro-casein. $CaCl_2$ is not a precipitant of pure caseinogen. Although one can make "curds and whey" in neutral milk, still the lay public have recognised the fact that acidity hastens the clot's appearance in that the rennet essences for culinary purposes are highly acid. Keeping in view the acidity of the preparations of rennet (upon which point Dr Ringer gives us scarcely enough information), I repeated his experiments with milk, diluted with water 1 in 4, which, as he alleges, does not clot with Martindale's faintly acid rennet. When this has been added to milk the acidity is very slight; if some of this mixture be boiled, and then either have added to it 1 m. acetic acid or 2 or 3 m. $CaCl_2$, a clot is obtained. Again, I always obtained a clot when using A.R. instead of M.R. with 1 in 4 milk. Both these rennets formed pro-casein, but it required either acetic acid or $CaCl_2$ to make the work of the faintly acid rennet manifest.

Further, on this point, the following experiments tell their own tale, R.C. + N.R. = 0, whereas R.C. + A.R. = clot, where R.C. = Ringer's pure caseinogen. Before the use of Ringer's test the following results would have corroborated the statement that "acidity favours while alkalinity inhibits *clotting*." Milk + A.R. = a clot at once, but milk rendered distinctly alkaline by the addition of $\frac{1}{10}$th of its volume of $NH_4.HO$ heated with M.R. gives no clot. If we boil some of this last and add 2 or 3 m. of $CaCl_2$ we get a clot, proving that alkalinity does not inhibit or antagonise the formation of pro-casein, but inhibits, without antagonising, the fall of casein. As far as my observations go, rennet is antagonised (? killed) in stage one by the presence only of concentrated acid added to one half the volume of the caseinogen-containing liquid; or by being heated to 100° C. Pro-casein formed in dilute solutions (*e.g.*, Ringer's caseinogen or milk 1 in 4) may not be precipitated by $CaCl_2$ alone, thus—R.C. + N.R. = 0, boil this, and add $CaCl_2$, still no clot, then add 1 in acetic acid when a clot appears. Again, acidity was the only differential factor in the following in both of which $CaCl_2$ was present—R.C. + $CaCl_2$ + N.R. = 0 whereas R.C. + $CaCl_2$ + A.R. = clot. Phosphoric acid, I find, forms a firmer clot than other acid used at the same dilution.

The Alkaline Earths as Precipitants of Casein.

Of these, calcic salts seem to be the readiest precipitants, only the soluble salts of calcium having any effect, which shows that this precipitation of pro-casein is not a mechanical process occurring round the particles of an insoluble salt as round foci, for the calcic phosphate, carbonate, and hydrate (the two former being added solid) have no precipitating power at all. Ringer, indeed, holds that CaH_2O_2 dissolves casein. The soluble salts of barium and strontium also precipitate pro-casein, the following of which were tested—baric chloride, nitrate, carbonate, strontic nitrate, and lactate. (The rather insoluble baric carbonate has not much precipitating power). Thus one cannot say it is the *metal* calcium which is effective in this precipitation, because both Ba and Sr can act vicariously; nor is it the *Cl* in association with it, seeing that $CaSO_4$, CaS, and $Ba2(NO_3)$, that is to say sulphuric acid, sulphur, and nitric acid act equally well. The salts of magnesium have little or no precipitating power; this is interesting in that magnesium is the lightest atomically of the group, so that the three alkaline earths, Ca, Ba, and Sr, which have so many properties in common, have this additional one—the power of precipitating pro-casein. Of course, $MgSO_4$ in saturation precipitates it just as it does caseinogen. The union of some calcic salt with pro-casein in the normal clotting of milk is alluded to later. Chlorides of the heavy metals precipitate it; NaCl does not. Chlorine water and pure iodine each precipitates pro-casein.

It is interesting that calcic orthophosphate, being the chief calcic salt native in milk, should be so inert when added pure to milk or "decalcified" milk. Since it appears in the various wheys, it must be soluble in milk-plasma; but may, of course, be in some kind of combination with the caseinogen too. I have failed to imitate any such solution of $Ca_32(PO_4)$ in egg-albumin.

III. THE CHEMISTRY OF CASEIN.

The product of rennet action, the curd, if allowed to dry, will of itself become a horny dull yellow mass of brittle character, and breaking with a vitreous fracture. Without any such pressure as is

used in cheese-making, it becomes like the hardest outside portions of a cheese. It has been classified as "coagulated proteid," and although it is such in origin, yet it departs markedly from the characteristics of that group as typified by "Fibrine." For instance, it is readily soluble in strong acids, *e.g.*, H_2SO_4, and alkalies, *e.g.*, KHO, forming a violet and a pink solution with each respectively. This is quite unlike fibrine. Further, casein may be dissolved (to a certain extent) in CaH_2O_2, from which solution it may be clotted by rennet, and this process may be repeated *ad infin.*—unlike fibrine, but like myosin. Ringer has insisted that casein is a chemical compound usually of a calcic salt, with the soluble form of casein: this I tried to verify by chemical analysis of the whey or filtrate from milk clotted by rennet, and milk coagulated by acetic acid. The results of one of these analyses are as follows:—*

	Total Ash in 50 c.c.	$Ca_32(PO_4)$ per cent. of Ash.
Acetic acid whey, .	·141 grm.	41·13 per cent.
Rennet whey, . .	·231 ,,	32·90 ,,

From which we see there is less calcic orthophosphate in 50 c.c. of rennet whey than there is in 50 c.c. of acetic acid whey—a difference which is a measure of the salt that has combined with the procasein to form the clot. This also shows that there is some *chemical* difference between the precipitation of caseinogen by an acid and the clotting of it by rennet—a difference able to be expressed, at least, in terms of the union of calcic phosphate. Some call this clot a caseate of calcium. Normally in milk it may be so; but, whatever view we take of its chemical nature, we must not forget that artificially we can produce a barium-casein or a strontium-casein with as great ease as a calcium-casein. Analysis also shows that there is less calcic phosphate in rennet whey than in the same volume of milk; again a proof that this salt has chemically united with the procasein.

I believe I have been able to observe a slight rise of temperature in milk to which rennet was added over that maintained in a specimen of pure milk; but, without further corroboration of this

* These analyses were kindly carried out for me by Mr William Lang, B.Sc., of the Chemical Department of Glasgow University.

point, would not care to insist upon it. I demur to the assertion that in the "ripening" of cheese we have an example of the direct conversion of proteid into fat, seeing that there is so much fat in cheese to begin with. What I believe happens is, that under bacterial fermentation the proteid (casein) is partly converted into caseoses or peptones, thus liberating much of the previously interstitially confined fat.

IV. On the Antagonism between Potassium Salts and Calcium Salts in the Clotting of Milk.

Ringer's words are: "The antagonism between calcium salts and sodium, potassium, and ammonium salts is limited to the precipitation of casein, and does not affect the chemical change from caseinogen to casein." * In this research I did not investigate the action of the sodium and ammonium salts. It must be remembered that Dr Ringer used—

(1) Milk diluted with water till it was 1 in 4 volumes.

(2) Martindale's preparation of rennet (reaction not stated), limeless, which did not clot the diluted milk.

(3) Only one salt of potassium, the chloride; at any rate, observations with no others are published in the above paper. Ringer, in other words, alleges that the presence of a salt of potassium antagonises the precipitation of procasein by a calcic salt. I therefore thought it well to study the general question of the effect upon the clotting of milk of as many potassium salts as I could lay my hands on, viz., twenty different salts. Believing, for reasons given already, that a neutral or faintly acid rennet will not bring about clotting in a solution containing such an amount of calcic salt as, with a more acid rennet, will cause clotting, I used a glycerine-extract of rennet of rather more distinctly acid reaction than Martindale's.

Half a test-tubeful of fresh neutral milk was dosed with 2 or 3 c.c. of the solution of the potassium salt (30 per cent. solution), and then received 5 m. of the rennet; the whole being thus heated to 50° C.

A control experiment was in each case made, wherein the con-

* *Journal of Physiology*, vol. xviii. p. 427; Nov. 16, 1895.

ditions were the same, except that, instead of the potassium salt, an equal volume of water was added to the milk.

The tubes containing the following salts yielded clots in as short a time (usually 10 to 15 minutes) as the clot took to appear in the control tube :—

Potassium	acetate	(alkaline).
„	sulphate	„
„	chlorate	„
„	nitrite	„
„	ferrocyanide	„
„	nitrate	(neutral).
„	chloride	„
„	bromide	„
„	iodide	„
„	phosphate	(acid).

In the tubes containing the following salts no clots were obtained at the end of 30′, and not until the alkalinity had been reduced or neutralised or the reaction made faintly acid by acetic acid :— Potassium, bicarbonate, bichromate, carbonate, chromate, cyanide, ferricyanide, permanganate,—these salts, except the second, are alkaline, some very decidedly so.

No clot was obtained when potassium oxalate was added to the milk,—the examination of this result will be taken up under decalcified milk. In no case was a salt of calcium *added* to the tubes, so that where a clot did occur it was in presence of a preponderating amount of potassium compared with calcium salt.

Thus, the group of potassium salts is sharply divided into two groups—(1) those faintly alkaline, neutral, or acid salts which do not antagonise the clotting of procasein in milk; and (2) a smaller group, all alkaline, which either inhibit the fall of casein, *i.e.*, delay its appearance beyond the normal time for clotting, or antagonise it altogether.

This inhibition or antagonism would seem to depend more upon the *reaction* of the potassium salt than upon any property in the salt as a salt of potassium.

It was not necessary in these experiments to apply Ringer's test as to whether procasein had been formed, because he holds that

the potassium salts do not antagonise this first stage of clotting; in several cases it was applied, one of which is given below.

Milk and K-cyanide + A.R. = 0: boiled and divided in three portions, to one of which was added 1 m. acetic acid, result a clot; to a second, 4 m. of $CaCl_2$ (10 % sol.), result a clot *at once* (showing that soluble casein was pre-formed); to a third, 1 m. acetic acid + 3 m. $CaCl_2$, result clot.

I made a large number of similar experiments with K-salts and caseinogen (Ringer's pure solution), but my results are too perplexing to be published at this time; the general result seems to point to a corroboration of the antagonism as alleged. Although some of the clots in the milk dosed with K-salts were jelly-like, others (and amongst them some of those in *alkaline* tubes) contracted to clots of very high tenacity.

V. The Case of Decalcified Milk.

There still remains the case of milk treated with potassium oxalate, which, if warmed with a not excessively acid rennet, will yield no clot. In order to deal with absolutely limeless substances, I added potassium oxalate to the rennet, and by centrifugalising a portion of it, obtained decalcified rennet. In like manner I centrifugalised the decalcified milk, obtaining a copious deposit of amorphous calcic oxalate, which passes through ordinary filter-paper. If a portion of the decalcified milk, which has been warmed with decalcified rennet for upwards of half an hour and has not clotted, be boiled and cooled, we find that a clot immediately appears on the addition of either a few m. of $CaCl_2$ (or other soluble salt of Ca, or a salt of Ba or Sr) or 1 m. acetic acid.

Throughout these processes we have (1) the native K-salts of the milk; (2) the K-phosphate, presumably formed in the double decomposition between the calcic phosphate and K-oxalate; and (3) some excess, doubtless, of uncombined K-oxalate. The failure of casein to appear as a clot with rennet in decalcified milk is therefore due not to the presence of K-salts but to the absence of calcium salts.

Decalcified milk allows its caseinogen to be precipitated by acetic acid and by all the proteid precipitants, as one might expect.

As to its precipitability by salts, I made a large number of observations tending to show that decalcified caseinogen is, on the whole, more unstable than normal caseinogen in milk—a result perhaps due to the molecular disturbance consequent upon breaking up the union of the proteid with the calcic phosphate.

VI. The Digestion of Milk "in vitro."

I had been often struck with the apparently unsatisfactory character of the artificial digestion of milk by means of ·2 % HCl and dry pepsine, in that no visible change appeared even after days on the water-bath. I thought it well to study the powers of coagulation of several artificial preparations, and their relative efficiency in digestion of milk as shown by the amount of caseoses or peptones formed.

Three-quarters of a test-tubeful of fresh milk received a pinch of Bullock's pepsina porci, an equal quantity of milk received a few m. ·2 % HCl, a third tube had both HCl and pepsine, a fourth ʒss. of Benger's liquor pepticus + a little HCl, a fifth a little of a fresh glycerine-extract of the pig's gastric mucosa, a sixth ʒss. of Benger's liquor pancreaticus + a little 1 % sodium carbonate.

At the end of 5′ on the water-bath at 40° C., I reported—

(1) In the tube with liquor pepticus there was a dense, tenacious, rope-like clot;

(2) In that with the liquor pancreaticus there was an exceedingly soft, friable clot, with as little cohesion as possible;

(3) In that with the glycerine-extract there was a fairly firm clot, not so good as in (1). In none of the others was there any *visible* change.

At the end of 30′ no further visible change was to be observed. After five hours, the tubes with the pepticus and glycerine-extract showed some signs of a partial solution of the clot in each, but it was by no means marked. That with the pancreaticus contained a turbid fluid: all the clot had vanished; the fluid was much more translucent than milk, and more so than the contents of any other tube. The superior translucency in this tube I attribute to the partial decomposition and saponification of the fat—a process which could not go on in any other tube. The second tube showed

an open flocculent precipitate; the first and third showed no change.

After eight days I tested the tubes for caseoses or peptones by the Biuret reaction. The pancreatic digestion yielded the test most frankly, next thereafter that with liquor pepticus, then that with HCl and pepsine, then that with pepsine alone (probably due to bacterial fermentation of caseinogen), and lastly a mere trace with the glycerine-extract.

My experiments with milk mixed with water or with CaH_2O_2, in order to render it more digestible, as it is alleged, tend to show that this property is entirely due to the greater laxity of the clot which forms when the solid particles of milk are, from any cause, separated from one another. Evidently the power of rennin for binding particle to particle is weakened when it has to act through a greater distance upon these particles: none of these clots in diluted milk has the absolute laxity of the pancreatic clot in milk.

Milk heated to 100° C. for 40 Minutes as a Method of increasing its Digestibility.

This I fully confirm. Sir William Priestley * called attention to Mons. Budin of Paris's observation that milk, not diluted and not boiled, but merely treated as above, yields a clot of digestibility superior to that produced in previously unheated milk.

I find the clot with Benger's liquor pepticus is, in the case of the heated milk, an exquisite jelly of great friability upon any such agitation as occurs with the gastric movements, whereas the tenacity and density of the clot by pepticus in ordinary milk would make the mass resist this breaking process for a much longer time. After some hours there was distinctly more proteose in the tube with the soft clot than in that with the hard.

On Boiled Milk.

The "scum" which forms on this is merely a surface dessication of caseinogen with possibly some lact.-albumin.

Milk boils at 104·5° C. I have corroborated the statement that

* *Brit. Med. Jour.* for 7th Dec. 1895.

fresh milk yields a blue colour with tincture of guaiac, while boiled milk does not.

I am unable to substantiate the statement of authors that boiled milk is "far more difficult to coagulate than unboiled milk." The clot in boiled milk so entirely fills the tube like a jelly, and separates from its whey so slowly, that unless the tube be examined very closely, and by inverting it from time to time, the time of the formation of the clot may easily be missed.

By "difficult," I presume, "takes a longer time" is meant, but I cannot find that the rennet clot occupies any longer time than in ordinary milk to do its work, whether it be added to cold milk previously boiled or to warm milk previously boiled. Authors state that this "difficulty" is due to some of the calcic phosphate having become insoluble. I can find none of it upon prolonged centrifugalisation of boiled milk.

The *fat of milk* may be very strikingly shown by adding to a large vessel of milk $\frac{1}{6}$th or so of its volume of a 1 % sol. of osmic acid. The mixture in about half an hour will have become jet black, and does not purify within a year.

The Wheys, Dialysates, and Filtrates.

In *rennet whey* there is some caseinogen, for there are yet many particles. It may be precipitated by heating the whey with $CaCl_2$ + and a m. or two of phosphoric acid, and it may be clotted by A.R. + $Cacl_2$ + phos. acid, the filtrate from which yields a proteid, but is perfectly free from particles.

Rennet whey treated with acetic acid gives a precipitate quite indistinguishable microscopically from any of the acid-precipitates of caseinogen.

The original clotting had therefore not comprised all the caseinogen.

Pressure whey (through a Berkefeld diatomaceous filter) has no particles, and will not clot. It has a proteid. It is faintly acid.

The *filtrate* (*without exhaustion*) through a clay filter has *no* proteid. (*Egg alb.* does not go through the pressure filter.)

All these wheys, including the *acetic acid whey*, have lactose

phosphates, chlorides, and sulphates, calcium, and, with the above exception, a proteid uncoagulable (lact.-alb.).

$MgSo_4$, or "salted whey." This has all these constituents, but it is incompetent to demonstrate sulphates in it.

If milk be dialysed into distilled water through vegetable parchment-paper, after twelve hours the fluid outside is faintly acid and slightly *opalescent*, but it shows no chemical evidence of proteid, save a very faint turbidity with Tauret's reagent. It is faintly acid in reaction. It has lactose and the salts (chlorides, phosphates, sulphates), and calcium.

Conclusions.

(1) Caseinogen is not, in the ordinary acceptation, in solution in milk-plasma, it is in the most intimate association with the fat; in all probability the particles consist of fat in direct ratio and of caseinogen in indirect ratio to their bulk: in this sense, caseinogen is "particulate."

The arguments that caseinogen is present as an envelope are still entitled to respect; but there is some evidence pointing to this proteid being interstitially associated with the fat.

(2) Both the clots and precipitates in milk and in "pure" caseinogen are more or less cohesive agglutinations of the pre-existing particles: there is no trace of fibrous structure.

(3) The so-called "pure solutions" of caseinogen are suspensions of particles upon whose presence depends their coagulability, in which respect they resemble diluted milk or concentrated whey. Their pressure-filtrates contain no proteid.

(4) Ringer's view of the clotting of casein being a twofold process receives confirmation in my experiments: it would be convenient to name the product of enzyme action upon caseinogen, procasein.

(5) The free acid present in many "essences" of rennet is, in many cases, and especially in the absence of lime, responsible for the precipitation of casein: it is a precipitant of procasein, not an adjuvant to enzyene action. Casein differs in many respects from the type of "coagulated proteid."

(6) Only the soluble salts of Ca, Ba, and Sr can precipitate procasein. They are concerned only with the second stage of clotting.

(7) There are chemical and physical differences between the precipitate of caseinogen and the rennet-clot (casein), one of which is that the latter has united with a larger quantity of $Ca_3 2(PO_4)$ than the former.

(8) One factor in the inhibition of clotting by certain K-salts is the high alkalinity.

(9) The absence of lime is a more powerful inhibitant than the presence of K.

(10) I have confirmed Budin's observations as to the superior digestibility of milk heated to 100° C.

Papers by Sydney Ringer, M.D., F.R.S., *consulted and alluded to.*

(1) "Regarding the Action of Lime Salts on Casein and on Milk," *Journal of Physiology*, vol. xi., 1890.

(2) "Further Observations on the Behaviour of Caseinogen," *ibid.*, vol. xii., 1891.

(3) "Further Observations on the Influence of Calcium Salts in promoting Heat Coagulation of Albumins," *ibid.*, vol. xiii., 1892.

(4) "Further Observations regarding the Antagonism between Calcium Salts and Sodium, Potassium, and Ammonium Salts," *ibid.*, vol. xviii., Nov. 1895.

January, 1896.

A Graphical Representation of Emotion as expressed in Rhythm. By Professor J. M. Dixon, M.A. (With a Plate.)

(Read May 4, 1896.)

An analysis of the versification of some of our nineteenth century poets has produced certain very interesting results that may be graphically represented. The subject of versification still demands more exact treatment, and any contributions which embody the results of real analytical treatment are of value.

It is well known that the chief objection to our iambic measure, which is the normal English measure, is its tendency to produce a monotonous pendulum-like tick-tick. If, on the other hand, we employ anapæstic measure, the triple movement to every foot demands too much exertion of the tongue. English verse possessing the finest cadences lies between the two extremes of a pure iambic treatment and a pure anapæstic treatment.

It may be laid down as a safe dictum that, in lyric verse at least, one anapæst in a line otherwise iambic constitutes it an anapæstic line. The poet who writes in iambics is not then free to introduce anapæsts. The expedient to which he will resort is a free use of the catch. We shall see that Gordon, in his exquisite ballad *The Sick Stock-Rider*, uses the catch, where emotion is at a high pitch, 13 times in 16 lines. A poet writing in anapæsts is free to use iambs where he will, and even the monosyllabic foot at the beginning of a line or after the cæsura. Browning, in his *Abt Vogler*, has actually one hexameter line (Stanza III. l. 7) with only 5 unaccented syllables, or one less than an iambic line can have. It is the shortest line in the poem and is placed next to an 11-syllabled line, the longest line he uses. There are only three of these 11-syllabled lines in this poem (III. 1, 2, 8). The average length of his line in the poem is 14·22 syllables.

Of the theoretically possible maximum of 96 unaccented syllables in an 8-lined anapæstic hexametric stanza, Browning never in any stanza gives us more than 71, or less than 61. The move-

ment hovers between these two extremes, and if represented graphically as a curve, is found to be a perfect index to the emotional quality. The poem may be described as a rendering in verse of the sensations of a musician in producing a masterpiece.

The curve (see Plate) represents five main phases of emotion.

Stanzas I., II., III., IV. are the overture, with a gradual heightening of emotion.

Stanzas V., VI., VII. give the supreme mountain-top height of delight in creation. The artist is "in heaven," and was "made perfect."

Stanza VIII. is heavy with the sadness of farewell. "The gone thing was to go."

Stanza IX. gives the comparative altitude (emotionally considered) of spiritual comfort: "There shall never be one lost good."

Stanzas X., XI., XII. are in the mood of cheerful acquiescence, at a point one degree lower than the overture is pitched.

In *Rugby Chapel* we have unrhymed anapæstic trimeters, with very frequent initial truncation of both unaccented syllables. The average number of unaccented syllables to the line is slightly under 4, and in the different stanzas ranges from a maximum of $4\frac{1}{6}$ to a minimum of $3\frac{2}{3}$. Here, again, the curve indicates the emotional quality. At the highest point, *a*, there is the happy recollection of the buoyant cheerfulness of the dead hero; at the nadir, *b*, there is a pessimistic outlook on a world given over to those who "chatter and love and hate," "achieving nothing."

Stanza I. Descriptive of time and place.

Stanza II. Happy reminiscences of the dead.

Stanza III. His death—with the sense of loss.

Stanza IV. Inquiry—"Where art thou?"

Stanza V. Assurance that he has a sphere of usefulness elsewhere.

Stanza VI. A pessimistic outlook on the world.

Stanza VII. Hope in a few chosen spirits—light in the darkness.

Stanza VIII. Safe arrival through difficulties of the few.

Stanza IX. Superior worth of the dead man.

Stanza X. Belief in the good men of former ages, through him.

Stanza XI. A eulogy of these noble spirits.

Stanza XII. The army of mankind—necessity of leaders.

Stanza XIII. Leaders, such as the dead man, are sure to be forthcoming.

Stanzas IX., X., XI. are intellectual and reflective, rather than emotional; hence, though not sad and almost despairing, like VI., they are sober and serious, approaching the iambic treatment.

In *The Sick Stock-Rider* the writer has chosen an iambic measure, heptameters and pentameters alternating, with alternate rhyme. The measure is used with much skill, especially in respect to the cæsura, which is used to break the mechanical beat of the iambs, and bring in anapæstic effects; but this is beside our present treatment. The element of expansion for emotional effect is found in the catch, which is remarkably frequent when the subject is exhilarating, while it sinks to zero where there is no liveliness. The points on the curve are obtained by estimating in fractions of 16 the ratio of the number of catches in each stanza to the number of lines composing it. In the case of the second stanza, which has 16 lines, the number 13 represents the real number of catches as well as the ratio $\frac{13}{16}$ths. On the other hand, in Stanza VII, which has only 8 lines, the real number of catches is 3, giving for ratio $\frac{6}{16}$ths. Similarly with the others. The following is a theme analysis:—

I. Overture. Weariness, brightening into cheerfulness.

II. Exhilarating reminiscences—"glorious gallops" and feats of derring-do.

III. Less exhilarating reminiscences.

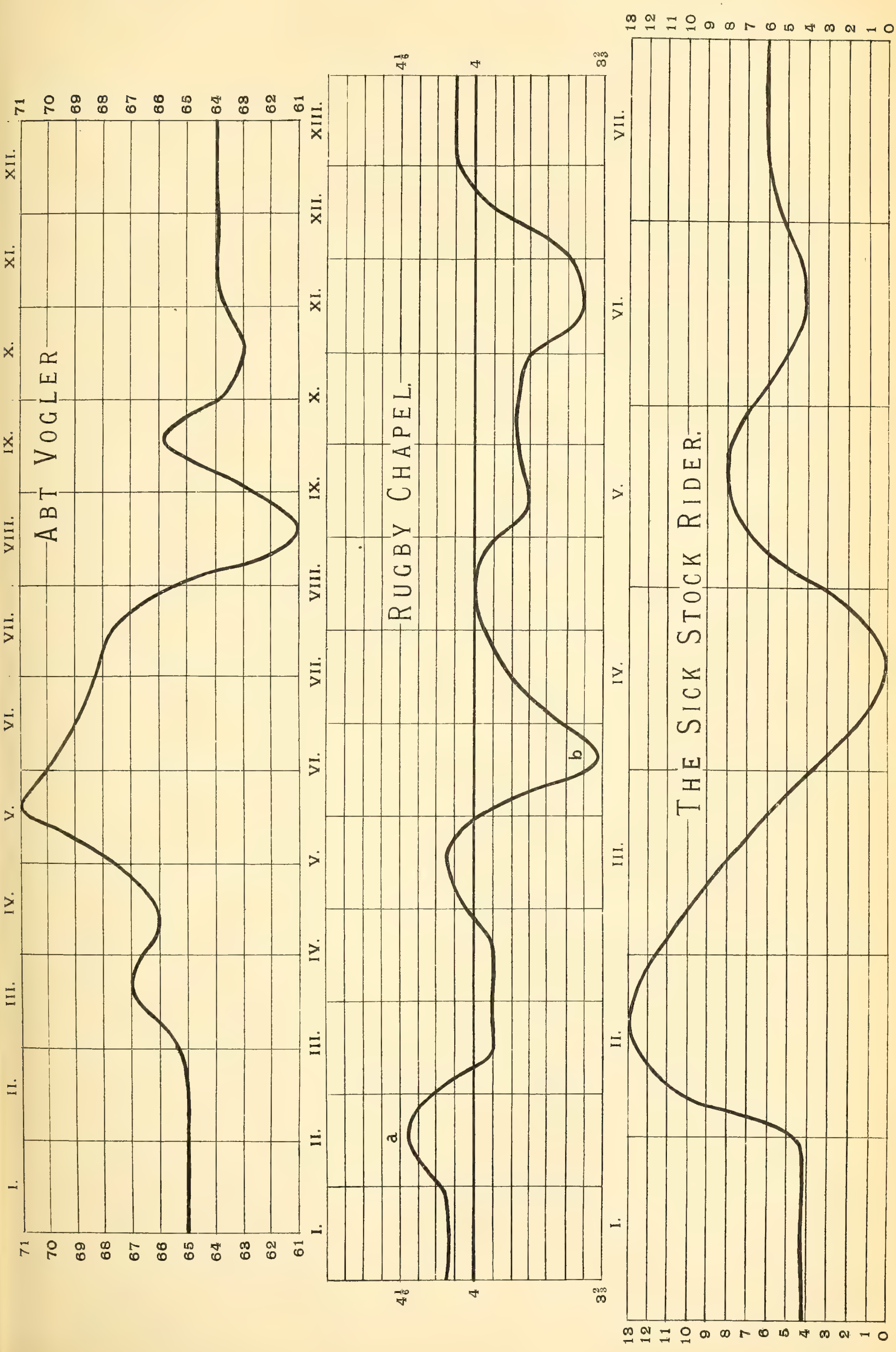
IV. Loneliness.

V. Less exhilarating reminiscences.

VI. The philosophy of life—resignation.

VII. A pleasant grave.

Professor Dixon on Emotion in Rhythm.



A. Ritchie & Son, Edin^r

Notes on Clouds. By John Aitken, Esq., F.R.S.

(Read May 4, 1896.)

There are two points connected with clouds on which I wish to make a few remarks. The first is on the classification of clouds, and the second on the manner in which certain forms of clouds are produced. It may be as well to remark at the outset that the observations are those of an "outsider," being in a department of meteorology to which the writer has given but little attention, and they have been written with the view of calling the attention of specialists, and getting their opinion on the subject.

It appears to me that in classifying clouds they ought first of all to be divided into two great classes. In the one class should be placed all clouds in the process of *formation*, and in the other those in the process of *decay*. The two classes might be called *Clouds in Formation* and *Clouds in Decay*. We may take Cumulus clouds as an example of the former, and Nimbus of the latter. My observations made in the clouds themselves have shown that there is a difference in the structure of these two classes of clouds. In clouds in formation the water particles are much smaller and far more numerous than in clouds in decay; and while the particles in clouds in decay are large enough to be seen with the unaided eye when they fall on a properly lighted micrometer, they are so small in clouds in formation that, if the condensation is taking place rapidly, the particles cannot be seen without the aid of a lens of considerable magnifying power. In the former case the number of particles falling per square millimetre is small, while in the latter they are so numerous that it is impossible to count them.

It appears that one good end might be served by adopting this classification. It would direct the attention of observers more to looking on the processes going on in *decay* for an explanation of many of the forms observed in clouds. In most books on clouds, when describing the different shapes of clouds, it is almost always assumed that they are in the process of *formation*, and the whole explanation of the shapes taken by the clouds is founded on this

supposition. Now it is very evident that very many clouds are in the process of decay, and their forms can only be explained by the processes going on under these conditions.

This brings me to the second point in this communication, namely, the manner in which ripple-marked cirrus clouds are produced. The explanation which has generally been accepted of the formation of this form of cloud is, that the ripple markings are due to the general movements of the air giving rise to a series of eddies the axes of the eddies being horizontal, and roughly parallel to each other. It is very evident that the air revolving round these horizontal axes, that is, in a vertical plane, will at the lower part of its path be subjected to compression, and at the upper part to expansion. The result of this will evidently be, supposing the air to be nearly saturated with moisture, a tendency for cloudy condensation to take place in the air at the upper part of its path, and it is this cloudy condensation in the upper part of the eddies that is supposed to produce the ripple-like cirrus; each ripple mark indicating the upper part of an eddy. One objection I have always felt to this explanation is, that it is difficult to imagine that the small amount of elevation and consequent expansion and cooling could give rise to so dense an amount of clouding as is generally observed. Any clouding produced in this way one would expect to be extremely thin and filmy. I have for the last few years made frequent observations of these clouds, and I have to admit I have never once seen them in the process of formation, or seen one appear in a clear sky. In all cases that have come under my observation, these ripple clouds have been clouds in decay. They are generally formed out of some strato-cirrus or similar cloud. When we observe these strato-cirrus in fine weather, it will be found that they frequently change to ripple-marked cirrus before vanishing. The process of their formation would seem to be: the strato-cirrus gradually thins away till it attains such a depth, that if there are any eddies at their level, the eddies break the stratus cloud up into parallel or nearly parallel masses, the clear air being drawn in between the eddies. It will be observed that this explanation requires the eddies, but not to produce the clouding, only to explain the breaking up of the uniform cirrus cloud into ripple cirrus.

One thing which supports this explanation is, that lenticular-

cirrus are frequently observed with ripple markings on one or more sides of them just where the cloud is thin enough to be broken through by the eddies. If we watch these lenticular-formed clouds under these conditions, we frequently see the ripple markings getting nearer and nearer the centre as the cloud decays; and at last, when nearly dissolved, the ripple markings will be seen extending quite across the cloud. It seems probable that "mackerel" and other cloud forms may be produced in the same way.

The shapes which these ripple cirrus assume are much more varied than is generally supposed. I lately observed a most interesting form in the south of France while the mistral was blowing strongly. There were a few cirrus in the sky at the time, and one of these was rapidly being broken up into irregular ripple forms, but at one point there was formed a most perfectly cylindrical-shaped piece, its length being about twenty times its diameter. The whirling effect of the eddy was very evident by the circular streaking of the clouding. Further, this cloud was evidently hollow, that is, the interior was filled with clear air as the cloud was thinnest along the axis, and it had all the appearance of a revolving tube of cloudy air.

It is not contended here that ripple clouds are never produced in the manner which has generally been accepted, only that so far as the writer's observations go they have never been observed forming in the manner supposed. It is hoped that others will put the explanation here offered to the test of observation, and it is principally with a view of getting others to repeat the observations that this has been written.

HYÈRES, 28*th March* 1896.

Note on the Digestion of Starch in the Stomach.

By W. G. Aitchison Robertson, M.D., D.Sc.

(Read April 6, 1896.)

Effect of Normal Gastric Secretion on Amylolysis by Ptyalin.

The following experiments were performed with normal human gastric secretion. After thoroughly washing out the stomach of a healthy man, several ounces of a dilute solution of Caffyn's liquor carnis were introduced into it. After the lapse of an hour the contents of the stomach were drawn off, filtered, and used instead of the pure acid solutions in former experiments. The acidity of the gastric fluid was due to inorganic acid and amounted to 0·15 per cent. hydrochloric acid.

10 c.c. gastric secretion, 10 c.c. 1 % starch solution, 10 c.c. saliva,	Acidity = 0·05 % HCl. At 38° C.	Starch unchanged after two hours.

This experiment shows that, in the stomach, with an acidity of the contents less than that even normally present in the gastric secretion, the action of ptyalin is wholly restrained.

Was the ferment merely inhibited from action by the acid, or was it destroyed?

To determine this, I took equal volumes of this gastric fluid, 1 per cent. starch solution, and saliva, and having mixed them, carefully neutralised the mixture with a solution of caustic potash, using very delicate test-papers to show the neutral point.

10 c.c. healthy gastric fluid, 10 c.c. 1 % starch solution, 10 c.c. saliva,	Carefully neutralised. At 38° C.	Starch wholly converted within ten minutes.

On examining the mixture shortly after neutralisation, the whole of the starch was found to have undergone conversion. It reduced Fehling's solution strongly, and contained 0·22 per cent. of reducing substance. This demonstrates that, with an acidity equal

to 0·05 per cent. hydrochloric acid, the action of ptyalin is restrained.

I performed similar experiments with the gastric fluid from a case of *chronic gastric catarrh*, the acidity of which was equal to 0·067 per cent. hydrochloric acid.

10 c.c. unhealthy gastric fluid,	Acidity	Starch unchanged
10 c.c. 1 % starch solution,	= 0·022 %	after two hours
10 c.c. saliva,	HCl.	at 38° C.

After being neutralised, however, the starch was completely converted within ten minutes, and the reducing substances present amounted to 0·23 per cent.

Does the acid merely restrain, or does it actually kill the ptyalin ferment, if exposed long enough to its influence?

By some authors it is affirmed that the former alone occurs, and that the conversive action of ptyalin again becomes potent on the gastric contents becoming alkaline after passing into the intestine. Others, again, state that the ferment is actually killed during its stay in the stomach.

Outside of the body I found that the following takes place. I placed in a vessel

10 c.c. gastric fluid (of acidity = 0·102 % HCl),	At 38° C. for
10 c.c. 1 % starch solution,	one hour.
10 c.c. saliva,	

At the end of an hour the starch had undergone no conversion. I then carefully neutralised the mixture and kept it at 38° C. for thirty minutes. It was then found to contain much erythrodextrin, soluble starch, and some unchanged starch. Substances reducing Fehling's solution amounted to 0·19 per cent.

In another experiment I left a similar mixture at a temperature of 38° C. for twenty-four hours before neutralising it. At the end of this period the starch had undergone no conversion. It was then neutralised and kept at the same temperature for thirty minutes, at the end of which time it contained much unchanged starch, some soluble starch and erythrodextrin, and 0·09 per cent. of reducing substances. After being kept in the warm chamber for two hours, it gave no coloration with iodine (showing that the

starch had been changed into the higher or achroodextrins), and contained 0·138 per cent. reducing substance.

These experiments show that, with the degree of acidity found in the above secretions, the ferment ptyalin is not killed, but its converting action is prevented. The diastatic action of the saliva seems also to become weaker the longer it is exposed to the influence of the acid, as is shown in the second case, where the ptyalin was exposed to the action of the acid for twenty-four hours. In this case the converting action was feeble and very slow compared with its action when exposed to the influence of the acid for one hour only.

I am inclined to believe, therefore, that, as the acidity of the gastric contents increases during digestion, so is the power of the ferment ptyalin diminished, till, at the end of digestion, it must be almost, if not entirely, destroyed and rendered incapable of resuming its function in the small intestine.

Digestion of Starch in the Stomach.

To determine whether the gastric secretion *per se* had any effect on starch, I performed the following experiments :—

I. *Healthy Gastric Secretion.*—I placed 30 c.c. of a 1 per cent. solution of starch in a vessel with 10 c.c. of gastric secretion from a healthy individual (the acidity of the latter was equal to 0·211 per cent. hydrochloric acid), and kept all at a temperature of 38° C. There was absolutely *no* change in the starch even after the lapse of two hours.

30 c.c. 1 % starch solution, 10 c.c. healthy gastric secretion = 0·211 % HCl,	At 38° C.	No conversion within two hours.

II. *Chronic Gastric Catarrh.*—With the secretion from an individual suffering from chronic gastric catarrh I performed a similar experiment and with a similar result.

30 c.c. 1 % starch solution, 10 c.c. unhealthy gastric secretion = 0·30 % HCl,	At 38° C.	No conversion after lapse of two hours.

III. *Pernicious Anœmia.*—A similar experiment was tried in this case, but with different results.

30 c.c. 1 % starch solution, 10 c.c. unhealthy gastric secretion, —acidity = 0·016 % as HCl,	At 38° C.	After one hour 0·6 % reducing substance present.

On testing the fluid one hour afterwards, 0·6 per cent. of reducing sugar was present. This appeared to be the maximum conversion, as the figure remained the same at the end of the second hour. We note, however, that in this case the acidity was practically *nil*, and the conversion was probably due to saliva swallowed accidentally.

My next experiments were made with starch *in the living stomach.*

I. *Healthy Digestion.*—After having thoroughly washed out the stomach of a healthy man, 250 c.c. of a 5 per cent. mucilage of starch were injected into his stomach, taking care that no saliva was included.

After the lapse of *one hour* the elastic tube was passed, and 72 c.c. of a turbid, whitish fluid were withdrawn. The total acidity was equal to 0·22 per cent. hydrochloric acid, and was composed entirely of this acid. The contents consisted of unchanged starch in large amount, tolerable quantities of soluble starch and erythrodextrin, along with a trace of reducing sugar.

After the lapse of *two hours*, 56 c.c. of clear, viscid fluid were syphoned off. The acidity was inorganic, and amounted to 0·17 per cent. There was very little starch present, and no erythrodextrin or sugar.

The erythrodextrin and sugar which were present at the end of the first hour had either been absorbed through the walls of the stomach or had been passed on through the pylorus.

II. *Chronic Gastric Catarrh.*—As in the preceding, 250 c.c. of the 5 per cent. starch mucilage were injected into the empty stomach.

One hour later, 96 c.c. of a whitish fluid were drawn off. The total acidity was equal to 0·127 per cent. hydrochloric acid. Much

unchanged starch was present, soluble starch and erythrodextrin in small amount, and a mere trace of sugar.

After an interval of two hours, 100 c.c. of a thick whitish-yellow fluid were obtained from the stomach. Organic and mineral acids were present in nearly equal amounts. The total acidity amounted to 0·225 per cent. Here, also, much unchanged starch was present, a small amount of erythrodextrin, and a trace of sugar.

III. *Pernicious Anæmia.*—250 c.c. of the 5 per cent. starch solution were injected into this patient's stomach with similar precautions.

After the lapse of one hour, 41 c.c. of clear thin fluid were withdrawn. The total acidity amounted to 0·01 per cent. as hydrochloric acid. Small amounts of unchanged and soluble starch were present, but no erythrodextrin. Reducing sugar amounted to 1 per cent.

After the lapse of two hours, 67 c.c. of thin yellowish fluid were obtained. The acidity was almost *nil*, amounting to 0·0036 per cent. A trace of starch was present, but erythrodextrin and sugar were both absent.

Gastric Digestion of Starch.

	Healthy.		Ch. Gastric Catarrh.		Pernicious Anæmia.	
Hours.	1	2	1	2	1	2
C.C. withdrawn, . .	72	56	96	100	41	67
Total acidity, . . .	0·22	0·17	0·127	0·225	0·01	0·0036
Unchanged starch, .	much	little	much	much	small amount	small
Erythrodextrin, . .	plenty	none	little	little	none	none
Sugar, per cent., .	trace	none	trace	trace	1 per cent.	none
Remarks, ...	turbid	viscid	whitish	thick	thin	thin

A glance at this table shows that, with the acidity of the healthy stomach or in pathological conditions where the acidity is not diminished, conversion of starch is soon brought to an end in the stomach. Whereas, in those conditions where the gastric juice has a feeble degree of acidity, the conversion of starch is

carried on to a great extent in the stomach, as is shown in the case of pernicious anæmia, where probably amylolysis was carried on to its full limit, and the products either at once absorbed or passed on into the duodenum.

In these experiments the starch was introduced directly into the stomach by means of the stomach-tube. In this way there was no admixture of saliva except what was swallowed subsequently. The table shows that, in cases where the normal acidity was not lessened, this swallowed saliva had little conversive action on the starch. It likewise shows that even a pure starch solution introduced into the stomach promotes the secretion of hydrochloric acid; a fact which, on *a priori* grounds, we should not have expected as being inimical to the digestion of amylaceous matters.

From the high degree of conversion which the starch has undergone in the stomach of the patient with pernicious anæmia, I should be inclined to think that this was due either to swallowed saliva or to a conversive ferment secreted by the gastric mucous membrane, as has been described as occurring in the stomach of the pig.

The above, however, is not the normal condition in which starchy food is received into the stomach. It is always mixed up with and accompanied by the products of salivary secretion.

In order to see what changes occur in starch solutions received into the stomach under such conditions, I performed the following experiments:—

Combined Effect of Salivary and Gastric Digestion on Starch.

Having thoroughly washed out the stomach, I allowed the following patients *to eat slowly* a fluid mucilage of boiled starch.

I. *Healthy Digestion.*—250 c.c. of boiled starch solution eaten slowly.

Fifteen minutes later I drew off 40 c.c. The total acidity was equal to 0·061 per cent. hydrochloric acid. Unchanged starch, soluble starch, and erythrodextrin were present in large amount, and reducing sugar amounted to 0·16 per cent.

I carefully neutralised this fluid, and kept it at a temperature of

38° C. to see if any further conversion would take place. After being kept thus for forty-five minutes, no further change had occurred. The ptyalin ferment had, in this case, been killed by this degree of acidity of the gastric contents, viz., 0·061 per cent. hydrochloric acid.

One hour after the ingestion of the starch solution other 45 c.c. were syphoned off. The total acidity equalled 0·102 per cent. as hydrochloric acid; much unchanged starch was present, along with traces of erythrodextrin and sugar.

This also was neutralised and kept at 38° C. for forty-five minutes. Neither in this case did any further change occur.

These examples illustrate that the normal acidity of the healthy gastric secretion not only restrains but soon kills the salivary ferment.

II. *Chronic Gastric Catarrh.*—250 c.c. boiled starch solution eaten slowly.

1. *Fifteen minutes* later 45 c.c. withdrawn.

Total acidity = 0·138 per cent. as hydrochloric acid.

Much starch, unchanged and in soluble form, present; also erythrodextrin, along with a faint trace of sugar.

This was neutralised and kept at 38° C. for three-quarters of an hour, but underwent no further change.

One hour after the starch was eaten, 38 c.c. were withdrawn.

Total acidity = 0·196 per cent.

A very small amount of starch was present, but no erythrodextrin or sugar.

This underwent no further change after being neutralised and kept at 38° C. for forty-five minutes.

2. Same patient.

250 c.c. starch mucilage supped.

Ten minutes later 80 c.c. of thin white fluid were syphoned off.

The total acidity reached 0·102 per cent.

Much starch was present, unchanged and soluble, some erythrodextrin, and sugar amounted to 0·4 per cent.

When neutralised and kept for three-quarters of an hour at 38° C., the sugar increased to 0·63 per cent.

One hour after eating the starch, 100 c.c. of turbid fluid were obtained.

The total acidity was equal to 0·175 per cent. as hydrochloric acid.

Much starch remained; erythrodextrin was present and 0·153 per cent. of sugar.

After being neutralised and kept at 38° C. for forty-five minutes, the sugar remained almost identical in amount (0·154 per cent.).

Combined Digestion of Starch by Saliva and Gastric Juice.

	Healthy Digestion.		Chronic Gastric Catarrh.			
Time in Minutes of Stay in Stomach,	15	60	15	60	10	60
No. of c.c. withdrawn,	40	45	45	38	80	100
Total acidity as HCl,	0·061	0·102	0·138	0·196	0·102	0·175
Starch, . . .	much	much	much	little	much	much
Erythrodextrin, .	much	traces	much	none	some	some
Sugar, per cent., .	0·16	traces	trace	none	0·4	0·153
After neutralisation. At 38°C. for 45 min.	no change	no change	no change	no change	0·63	0·154

From this we learn that even a short stay in the stomach during the digestion of a simple carbohydrate suffices to kill the ferment ptyalin. This ferment acts with great rapidity, and the amount of starch which was found in the stomach converted into dextrins and sugar had probably undergone this change during the process of mastication and swallowing, and for a short period after its entrance into the stomach.

In the case of the dyspeptic, the ferment was not killed after a stay of ten minutes in the stomach, and was still able to continue its amylolytic action in a less acid medium. A stay of five minutes longer, however, in the stomach, during which time the amount of acid had increased, was enough to destroy the ferment, for, after being neutralised, no further amylolytic action took place.

We may therefore state that the diastatic ferment of the saliva is killed by a certain degree of acidity of the gastric contents (varying from 0·06 to 0·1 per cent. hydrochloric acid), and, depending on the rapidity of production of this acid, it continues active in the stomach until this limit is reached.

The sugar formed from starch conversion during this period must be at once absorbed in healthy conditions, as it is not found in the stomach at a later stage in digestion.

On the Seasonal Death-rate from Certain Diseases in Edinburgh during the Period 1878–94, with Remarks on the Relation between Weather and Mortality. By R. C. Mossman, F.R.S.E., F.R. Met. Soc. (With a Plate.)

(Read March 2, 1896.)

This inquiry embraces the seventeen years beginning with 1878 and ending with 1894, the material utilised being the weekly mortality returns published by the Registrar-General for Scotland. Particulars are given in these returns of the number of deaths from small-pox, measles, scarlet fever, diphtheria, whooping-cough, diarrhœa, fever, croup, laryngitis, bronchitis, pneumonia, and pleurisy. Some of the diseases are classed together, viz., typhus and typhoid fever, croup and laryngitis, and bronchitis, pneumonia, and pleurisy. The data discussed also embraces returns of deaths from all causes, deaths of infants under one year of age, and of persons aged sixty years and upwards. The deaths have all been entered as having occurred during the calendar weeks in which they were registered, but as notification usually follows decease by several days, a slight element of error is introduced. Since the returns do not give any information as to the duration of the illness, we are unable to ascertain whether specific weather types are in casual relationship to disease. There are every week a number of deaths from uncertified and unspecified causes. These omissions are subsequently rectified, but their presence in the returns does not affect the general results of the investigation. It is our intention merely to discuss some of the broader results of the inquiry, the chief object of which is to give a first approximation to the seasonal death-rate of the diseases under review. In discussing the relation between weather and disease, it is well to keep in mind the fact that different diseases exhibit varying degrees of sensitiveness to weather influences, and that climatic changes take some little time to affect the human subject.

The meteorological data for the period under review were sup-

plied to the Registrar-General by the Edinburgh observers of the Scottish Meteorological Society. The observations were taken at Cumin Place until 1886, when the station was changed to Blacket Place, where they are still carried on.* These two stations are only half a mile from each other and at the same height above mean sea-level, the readings being thus strictly comparable. In order to supplement the information given weekly to the Registrar-General, monthly means of the more important climatic elements have been computed for the seventeen years, thus enabling a tolerably complete representation of the climatic features to be given. Some of the monthly means have been compared with their averages for long periods, so that some idea might be obtained as to the departures from the normal during the period under consideration (see Table III.). Tables for each week, showing the cumulative variability of temperature, have been computed. These values are found by taking the day to day differences in the mean temperature † of successive days and adding up the seven daily values during the week. The variability of temperature is largely the result of changes in the direction of the wind. No information is available for the period under review as to the height of the underground water or the percentage of gases, as carbonic acid gas and other deleterious substances in the underground air. This is a matter for regret, as it is generally believed that the state of public health depends in no inconsiderable degree on the chemical and bacteriological condition of the superficial layers of the earth.

The curves which accompany this paper were prepared as follows:—The number of deaths from each disease having been extracted for each week during the seventeen years, the values were added up and divided by fifty-two in order to obtain the average weekly death-rate (see Table I.). The percentage excess or defect of each week's mean from the average of the whole year was then calculated, and the values smoothed by Bloxam's method. This process consists in assuming the average, for example, of the 2nd week of January to be not the actual average of that week,

* For some years after 1886, observations were also made for the Scottish Meteorological Society by Mr Buchanan at Oswald Road.

† The mean temperature was assumed to be the arithmetical mean of the maximum and minimum readings.

but the mean of the 1st, 2nd, and 3rd weeks, the average of the 3rd week being the mean of the 2nd, 3rd, and 4th weeks, and so on (see Table II.). The values thus obtained were then plotted. It may be stated that a 53rd week had in one or two years to be introduced by the Registrar-General in order to bring his returns into approximate agreement with the calendar. When the introduction of a 53rd week was necessary the data given in the first weekly return for the year in question were not tabulated.

Fig. 1.—*Deaths of Both Sexes at all Ages and from all Causes.*

The characteristic features of the mortality curve, which is tolerably constant from year to year, are (1) a period of high death-rate from about the middle of November to the end of January; and (2) a long-continued minimum in summer, extending from the beginning of July to the end of September. During the former period temperature falls rapidly, with a considerable increase in rainfall and humidity. The variability of temperature is at a maximum, the general atmospheric conditions being thus raw and inclement. During the time characterised by low mortality, air temperature is high, humidity low, and temperature variability at a minimum. The rapid fall in the death-rate during the four weeks ending with the middle of February is of interest, as is the general retardation of any further improvement in the mortality returns until the end of May, thus confirming the general impression that the easterly winds of spring exercise a prejudicial influence on the health of the community.

Fig. 2.—*Deaths of Infants under 1 Year of Age.*

Infantile mortality has its primary maximum from November to January, a secondary maximum due to diarrhœa being a marked feature during the month of August. The minimum extends over the six months ending with July, but is not such a pronounced characteristic of the curve as the maximum. The absolute minimum is reached about the end of June, the absence of zymotic and bronchial affections at the time being of interest in this connection.

Fig. 3.—*Deaths of Persons Aged* 60 *Years and Upwards.*

This curve shows the same seasonal variation as that of deaths from all causes, the maximum fatality being from November to January, and the minimum from July to September. The winter maximum is apparently associated with the prevalence of respiratory complaints, from the attacks of which so many old people succumb.

Fig. 4.—*Measles.*

This, as regards the maximum, is rather an irregular curve, there being three well-defined periods showing a high death-rate, viz., the end of December, the middle of February, and the end of March. The climatic features of this extended period are a rather low temperature, small rainfall, but high humidity, although steadily diminishing. Towards the close of May the mortality greatly diminishes, the absolute minimum occurring in August.

Fig. 5.—*Whooping-Cough.*

This is essentially a complaint of spring and early summer, the mortality curve being above the mean from the second week of February to the beginning of July, and below it during the remainder of the year. This disease also exhibits three well-defined maxima, the first two of which coincide approximately with the corresponding maxima from measles. The primary maximum, however, occurs about five weeks after that of measles. Hence, whooping-cough is most fatal when pressure and temperature are high, and humidity at the annual minimum conditions intimately connected with the prevailing easterly winds. The disease is at a minimum in November. The climatic features of the months when the fatality from this disease is small are a low temperature, considerable rainfall, and great humidity, with an almost complete absence of winds from the east.

Fig. 6.—*Scarlet Fever.*

Scarlet fever is above the average from the middle of September to the end of February. It increases with great rapidity in October, reaching a maximum in the middle of November, when a rapid fall takes place, the disease in an epidemic form being virtually over

by the middle of December. The lowest death-rate occurs in the middle of July.

FIG. 7.—*Croup and Laryngitis, and* FIG. 8, *Diphtheria.*

The curves for the above diseases being based on only ten years' observation, it would be premature to draw conclusions, further data being required to define the seasonal variations.

FIG. 9.—*Diarrhœa.*

The death-rate from diarrhœa is above the mean from June to October, being at a maximum about the beginning of September, when the mortality is over 150 per cent. above the annual average. The minimum occurs in March.

FIG. 10.—*Bronchitis, Pneumonia, and Pleurisy.*

The curve of these three diseases of the respiratory organs closely follows that of deaths from all causes, being above the mean from October to April, and below it during the other half of the year. An intimate relationship exists between the mortality from respiratory diseases and the variability of temperature as deduced by the method described; the more changeable the weather the greater the mortality. This will be clearly seen on examining the curves of mean temperature and mean variability of temperature, and comparing them with the curve of deaths from the three diseases under review. The most changeable weather, it will be seen, occurs not during the cold spell at the beginning of January, but at the end of November, when the mortality from respiratory diseases is at a maximum. This is not a local phenomenon, the greatest death-rate from these diseases in London being observed at this time. In order to show more clearly the prejudicial effect of changeable weather, we have examined the weekly mortality returns for these diseases for the ten years 1885–94. We have corrected the values so as to allow for increase in population,* the returns for each year being thus strictly comparable with each other. The

* For some years prior to the taking of the census in 1891, the population of Edinburgh had been over-estimated, but Dr Blair Cunynghame kindly sent me the corrected numbers for each year.

result of this investigation is that, when the cumulative variability of temperature did not exceed 14°, the weekly number of deaths amounted to only 16·5, but when the variability was above 28° the deaths numbered 22·5, an increase of 36 per cent. The following are the values :—

	Below.			Above.
Variability, . .	15°	15°–21°	22°–28°	28°
Deaths per week, .	16·5	18·7	19·9	22·5

It has been long known that the mortality from the three diseases under review varies inversely as the temperature. This applies to all but the lowest temperatures, as will be apparent from the following table, the returns being for the ten years specified above.

	Below.						Above.
Temperature, .	36°	36°–40°	41°–45°	46°–50°	51°–55°	56°–60°	60°
Deaths per week,	25·3	26·0	22·4	17·4	13·5	13·1	11·6

The lowest mortality from bronchitis, pneumonia, and pleurisy occurs at the end of August, after a period of high temperature and small variability.

FIG. 11.—*Fever.*

The returns include deaths from the following fevers, viz., typhoid, typhus, relapsing, cerebro-spinal, simple and ill-defined. The mortality * from the latter diseases is so small that the deaths from them may be neglected. The usual autumnal rise of typhoid is well marked, the maximum fatality occurring at the end of November.

Small-Pox.

Nearly all the cases of small-pox having occurred during only one epidemic, that of 1894, it is not possible to construct a curve for this disease, but half of the deaths recorded took place in December of that year.

Violence.

The curve of deaths from violence is extremely irregular, and

* The following were the deaths recorded in the county of Edinburgh during the year 1893 :—Typhoid, 74 ; typhus, 6 ; relapsing, 0 ; cerebro-spinal, 0 ; simple, 3. (See *Registrar-General's Detailed Report*, pp. 54 and 55.)

has not been reproduced. The only pronounced feature is a great increase during the first week of the year, the mortality being 78 as compared with 41 during the week preceding, and 49 in the second week of the year.

Meteorology.

The general climatic features during the seventeen years will be apparent from the tables appended, some of which are graphically shown in figs. 12 to 14. A detailed report on the meteorology of Edinburgh is deferred until the completion of a memoir dealing with the data deduced from observations which extend back to the year 1764.

Death-rate from some Diseases in Edinburgh as compared with London.

A comparison of a few diseases shows that great differences exist in the time of maximum and minimum fatality in Edinburgh as compared with London. Our information for the English metropolis is derived from Dr Buchan's and Sir Arthur Mitchell's classical memoir (*Jour. Scot. Met. Soc.*, vol. iv. p. 187), the years there discussed being the period 1845–74. Whether the results would have been different for the same seventeen years as those dealt with for Edinburgh we are unable to say. The following table shows the weeks of maximum and minimum fatality, with the percentage excess or defect of the values from the annual mean for the diseases which we have been able to compare :—

	Maximum.			Minimum.			Percentage Difference from Annual Mean.			
							Maximum.		Minimum.	
	Lond.	Edin.	Diff.	Lond.	Edin.	Diff.	Lond.	Edin.	Lond.	Edin.
	Weeks.	Weeks.		Weeks.	Weeks.					
Deaths from all Causes, . . .	2	2	0	24	33	− 9	+ 14	+ 19	−15	−16
Bronchitis, Pneumonia, and Pleurisy, ...	49	49	0	33	35	− 2	+ 59	+ 54	−50	−49
Measles, . . .	5	14	−9	39	35	+ 4	+ 43	+ 62	−32	−72
Whooping-Cough,	15	20	−5	38	45	− 7	+ 43	+ 65	−44	−63
Scarlet Fever, .	44	46	−2	14	29	−15	+ 60	+ 90	−35	−51
Diarrhœa, . .	31	36	−5	13	9	+ 4	+307	+151	−75	−57
Typhoid Fever, .	46	49	−3	21	30	− 9	+ 41	+ 42	−33	−42

Hence, the maximum fatality from the five zymotic diseases under review occurs earlier in London than at Edinburgh by from two to nine weeks. The minimum death-rate also occurs earlier, except in the case of measles and diarrhœa, which are both a month later in London than in Edinburgh. The most noticeable feature in London, as compared with this city, is the very early date of the lowest death-rate from all causes—nine weeks earlier than in Edinburgh. This is clearly due to the great increase in infantile diarrhœa, which begins in London about the middle of June; whereas in Edinburgh, deaths from this disease do not increase in a marked degree until about the end of July, and are so few in number that the general death-rate remains practically unaffected.

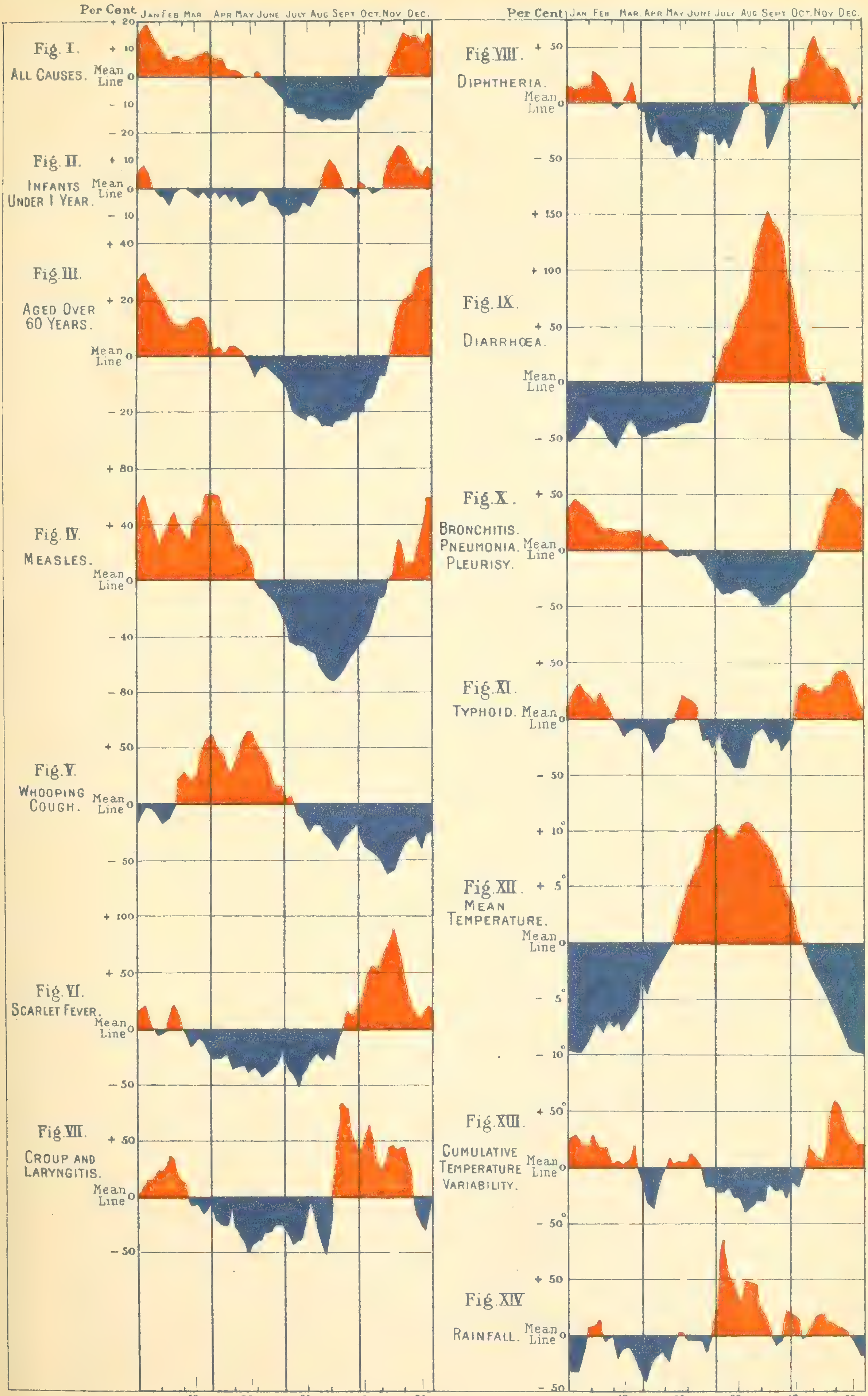
TABLE I.—*Showing for Edinburgh the Total Number of Deaths from various Diseases, as Published by the Registrar-General in his Weekly Reports from* 1878–1894, *with certain Meteorological Data for the same Period.*

Week.	From All Causes.	Infants under 1 Year.	Aged over 60 Years.	Small-pox.	Measles.	Scarlet Fever.	Diphtheria.	Whooping-cough.	Fever.	Diarrhœa.	Croup and Laryngitis.	Bronchitis, Pneumonia, and Pleurisy.	Violence.	Mean Temperature.	Cumulative Variability of Temperature.	Mean Rainfall.	Month.
														°	°	Ins.	
1	1953	352	472	0	73	46	17	49	23	28	11	426	78	36·7	412	·41	Jan.
2	1877	316	461	1	67	30	16	52	31	18	11	458	49	36·6	352	·29	,,
3	1843	299	478	0	51	34	10	56	27	30	13	456	38	37·7	354	·40	,,
4	1761	290	433	1	52	29	18	46	18	35	11	388	42	39·1	384	·68	,,
5	1695	281	426	0	42	23	10	44	29	33	13	405	49	39·3	342	·61	Feb.
6	1713	287	417	0	58	37	16	44	24	23	13	375	50	40·0	395	·42	,,
7	1681	268	399	1	61	40	16	50	23	30	15	353	46	37·5	315	·75	,,
8	1728	326	402	0	58	34	11	65	24	21	11	357	52	42·3	341	36	,,
9	1715	295	407	0	43	31	16	79	18	14	9	375	41	38·5	344	·45	March
10	1662	276	391	1	56	22	13	59	20	25	14	336	42	39·6	341	·47	,,
11	1712	311	423	2	47	26	15	56	17	39	5	364	39	39·8	363	·47	,,
12	1744	282	418	1	67	36	13	82	20	30	9	372	43	41·4	365	·48	,,
13	1647	294	379	0	58	20	17	90	22	21	12	328	44	41·0	369	·38	,,
14	1732	293	387	0	68	18	6	78	17	25	8	354	55	43·1	248	·29	April
15	1694	302	354	1	50	29	12	85	16	26	5	340	64	43·4	295	·35	,,
16	1661	273	387	0	71	20	8	71	14	26	11	331	39	45·6	327	·58	,,
17	1600	299	360	0	50	20	10	72	21	24	8	316	39	45·2	311	·41	,,
18	1625	277	378	3	47	19	7	58	19	31	10	325	36	46·7	322	·37	May
19	1646	292	393	0	50	22	7	86	22	26	3	291	59	47·9	366	·47	,,
20	1544	260	348	2	51	21	11	96	21	28	6	297	38	50·3	324	·67	,,
21	1584	287	326	0	39	14	3	74	24	32	6	274	45	51·6	348	·48	,,
22	1643	306	346	1	38	25	8	86	30	32	6	300	47	51·9	336	·42	June
23	1620	291	369	4	34	16	9	78	19	28	7	297	53	53·1	333	·62	,,
24	1533	289	334	0	35	16	11	61	21	31	8	285	37	55·4	317	·47	,,
25	1483	256	342	3	33	27	7	71	12	40	6	251	35	55·6	308	·47	,,
26	1469	287	347	0	32	18	8	52	19	45	9	218	60	57·7	288	·43	,,
27	1426	258	311	4	21	27	12	62	17	56	8	191	50	57·8	294	·74	July
28	1363	262	296	1	24	14	4	47	19	53	4	185	42	57·2	308	·80	,,
29	1427	288	280	3	17	14	10	60	11	55	6	181	53	58·3	258	1·07	,,
30	1363	266	298	1	19	16	9	37	14	77	9	191	45	57·0	303	·58	,,
31	1355	294	279	0	23	29	8	44	13	63	8	200	48	57·7	300	·61	Aug.
32	1354	283	298	0	16	20	19	44	11	81	12	190	46	58·1	290	·74	,,
33	1362	293	281	0	16	21	11	43	20	89	3	198	49	57·3	320	·90	,,
34	1327	328	255	1	9	25	22	44	23	99	5	173	49	57·2	265	·60	,,
35	1385	334	291	0	8	25	6	32	14	112	8	151	48	56·0	298	·80	Sept.
36	1353	322	279	0	13	17	8	37	21	113	12	140	45	55·5	288	·52	,,
37	1322	310	275	2	12	38	9	31	16	119	20	173	42	55·3	334	·41	,,
38	1386	274	294	0	15	37	11	42	15	97	22	179	48	52·5	249	·54	,,
39	1367	295	289	0	20	30	15	44	17	104	11	192	46	52·0	305	·57	,,
40	1427	297	309	2	20	34	18	45	20	68	17	202	54	49·4	275	·78	Oct.
41	1456	317	296	0	22	46	12	29	28	77	19	214	45	48·6	306	·52	,,
42	1524	293	337	1	28	49	14	29	30	54	18	261	52	46·4	316	·52	,,
43	1426	271	311	0	30	46	23	37	23	48	7	251	30	45·3	366	·52	,,
44	1615	321	369	0	40	45	19	24	26	44	13	299	51	44·2	421	·59	Nov.
45	1586	300	336	3	33	57	19	15	28	44	17	384	47	43·8	298	·67	,,
46	1681	348	405	0	41	58	16	26	23	49	12	398	41	40·9	382	·56	,,
47	1839	361	444	4	46	56	17	26	28	50	14	472	58	41·4	408	·63	,,
48	1796	328	430	8	62	41	16	30	28	27	16	458	43	40·0	410	·57	Dec.
49	1861	330	449	7	22	37	17	48	31	26	13	492	45	37·8	390	·53	,,
50	1770	308	464	2	48	37	15	32	29	22	7	468	42	37·1	396	·60	,,
51	1814	310	492	5	61	29	12	34	23	26	5	445	53	37·3	369	·51	,,
52	1710	292	466	1	43	31	11	35	21	23	10	403	41	37·6	384	·41	,,
Means,	1593	298	366	1	39	30	13	53	21	46	10	307	47	46·9	332	·53	

TABLE II.—*Showing the Smoothed Percentage of Excess or Defect of the Death-rate of each Week of the Year as compared with the General Average of the Fifty-two Weeks, with the Departure from the Annual Mean of certain Meteorological Elements at Edinburgh during the* 17 *years* 1878–94. N.B. *The heavy type indicate a Mortality above the Average, and the light type below it.*

Week.	From All Causes.	Infants under 1 Year.	Aged over 60 Years.	Measles.	Scarlet Fever.	Diphtheria.	Whooping-cough.	Fever.	Diarrhœa.	Croup and Laryngitis.	Bronchitis, Pneumonia, and Pleurisy.	Mean Temperature. °	Cumulative Variability of Temperature. %	Mean Rainfall. %	Month.
1	16	7	28	55	19	13	*15*	20	*52*	7	39	*9·9*	25	*32*	January
2	19	8	29	62	22	10	*1*	30	*47*	17	45	*9·9*	28	*33*	,,
3	15	1	25	45	3	13	*3*	22	*42*	17	41	*9·1*	22	*15*	,,
4	11	*3*	22	22	*4*	13	*9*	18	*30*	23	35	*8·2*	19	6	,,
5	8	*4*	17	30	*1*	28	*17*	13	*35*	23	27	*7·4*	27	7	February
6	6	*7*	13	38	11	23	*14*	22	*38*	37	23	*8·7*	17	13	,,
7	7	*2*	11	50	23	18	*0*	13	*47*	30	18	*7·0*	17	*4*	,,
8	7	*0*	10	38	16	3	23	3	*53*	17	18	*7·5*	1	*2*	,,
9	6	*0*	10	32	*3*	*5*	29	*2*	*57*	13	16	*6·8*	3	*21*	March
10	6	*1*	12	25	*12*	*2*	23	*14*	*45*	*7*	16	*7·6*	1	*13*	,,
11	7	*3*	13	45	*6*	5	25	*11*	*33*	*7*	16	*6·6*	6	*11*	,,
12	7	*1*	12	45	*9*	16	43	*7*	*36*	*13*	15	*6·2*	19	*17*	,,
13	8	*3*	8	62	*18*	*8*	60	*7*	*46*	*3*	15	*3·1*	*4*	*29*	,,
14	6	*1*	2	62	*25*	*10*	62	*13*	*49*	*17*	11	*4·4*	*30*	*38*	April
15	6	*3*	3	60	*25*	*34*	50	*28*	*46*	*20*	12	*2·9*	*35*	*25*	,,
16	3	*2*	*0*	45	*23*	*23*	46	*21*	*46*	*20*	7	*2·2*	*15*	*17*	,,
17	2	*5*	3	42	*34*	*36*	28	*16*	*42*	*3*	6	*1·1*	*2*	*15*	,,
18	2	*3*	3	25	*32*	*38*	38	*1*	*42*	*30*	1	*0·3*	8	*23*	May
19	1	*7*	2	25	*31*	*36*	54	*1*	*39*	*37*	*1*	1·4	4	*5*	,,
20	*0*	*6*	*3*	20	*36*	*47*	65	7	*39*	*50*	*6*	3·0	5	2	,,
21	*0*	*5*	*7*	10	*33*	*44*	65	20	*35*	*40*	*5*	4·4	6	1	,,
22	1	*1*	*5*	*5*	*38*	*49*	53	17	*35*	*37*	*5*	5·8	14	*5*	June
23	*0*	*1*	*4*	*7*	*36*	*29*	44	12	*35*	*30*	*4*	6·9	8	*5*	,,
24	*3*	*6*	*5*	*12*	*34*	*31*	34	*18*	*29*	*30*	*9*	8·1	*0*	*6*	,,
25	*6*	*7*	*7*	*15*	*32*	*33*	17	*18*	*17*	*23*	*18*	9·7	*17*	*15*	,,
26	*8*	*10*	*9*	*25*	*20*	*31*	17	*25*	2	*23*	*28*	10·1	*17*	3	,,
27	*11*	*10*	*13*	*32*	*34*	*39*	1	*13*	13	*30*	*35*	10·7	*21*	25	July
28	*11*	*9*	*20*	*45*	*39*	*34*	7	*27*	20	*40*	*39*	9·9	*22*	82	,,
29	*13*	*9*	*21*	*48*	*51*	*41*	*10*	*32*	35	*37*	*39*	9·7	*17*	54	,,
30	*13*	*5*	*22*	*48*	*34*	*31*	*12*	*42*	42	*23*	*37*	9·8	*26*	42	,,
31	*15*	*6*	*21*	*50*	*27*	*8*	*23*	*42*	60	*3*	*36*	10·7	*27*	23	August
32	*15*	*3*	*22*	*52*	*22*	*3*	*19*	*32*	69	*17*	*35*	10·8	*56*	48	,,
33	*16*	1	*25*	*62*	*26*	33	*19*	*16*	96	*33*	*38*	10·6	*35*	47	,,
34	*15*	7	*25*	*70*	*21*	*0*	*27*	*11*	119	*47*	*43*	9·9	*31*	46	,,
35	*15*	10	*25*	*72*	*25*	*8*	*31*	*8*	136	*17*	*49*	9·3	*30*	21	September
36	*15*	8	*23*	*70*	*11*	*41*	*39*	*20*	151	33	*49*	8·7	*14*	9	,,
37	*15*	1	*23*	*65*	2	*29*	*33*	*18*	140	80	*46*	7·5	*20*	8	,,
38	*15*	*2*	*22*	*57*	16	*10*	*28*	*25*	133	77	*40*	6·4	*18*	5	,,
39	*12*	*3*	*19*	*52*	12	13	*19*	*19*	96	50	*37*	4·4	*27*	20	,,
40	*11*	2	*19*	*45*	22	16	*27*	3	81	40	*34*	3·1	*13*	18	October
41	*8*	1	*15*	*40*	43	16	*37*	24	45	63	*26*	1·2	*16*	15	,,
42	*8*	*2*	*14*	*30*	56	26	*43*	30	30	47	*21*	*0·1*	*1*	*2*	,,
43	*4*	*1*	*7*	*15*	55	44	*46*	27	6	27	*12*	*1·6*	21	3	,,
44	*3*	*0*	*7*	*12*	64	57	*53*	23	*2*	23	1	*2·5*	11	13	November
45	2	9	1	*2*	78	39	*63*	23	*1*	40	17	*3·9*	7	15	,,
46	7	13	8	2	90	34	*61*	27	4	43	36	*4·9*	5	18	,,
47	11	16	17	28	72	26	*57*	27	*8*	40	44	*6·1*	46	11	,,
48	15	14	21	10	48	29	*37*	40	*26*	43	54	*7·2*	58	9	December
49	14	8	22	12	27	24	*33*	42	*46*	20	54	*8·6*	51	7	,,
50	14	6	28	12	14	13	*30*	33	*47*	*17*	52	*9·5*	31	3	,,
51	11	2	30	30	8	*3*	*39*	17	*49*	*27*	43	*9·6*	19	*5*	,,
52	15	7	31	50	19	2	*27*	7	*45*	*13*	38	*9·7*	19	*17*	,,

R. C. MOSSMAN ON THE SEASONAL DEATH-RATE FROM CERTAIN DISEASES IN EDINBURGH.



McFarlane & Erskine, Lithrs Edinr.

TABLE III.

Showing for the period 1878-94 the Mean Monthly and Annual Values of the Meteorological Elements observed at Edinburgh.

Showing the Departure of the Meteorological Values 1878-94 from the Means of Longer Periods. *N.B.*—Heavy type indicates that the mean of the 17 years 1878-94 was above the long average; light type that it was below it.

	Mean Barometric Pressure at 32° and M.S.L.	Temperature in Shade 4 Feet above Grass. Mean Maximum.	Mean Minimum.	Mean Daily Range.	Mean Daily Variability.	Mean Temperature.	Relative Humidity Saturation=100.	Rain.* Total Fall.	Number of Days with 0·01 inches or more.	Wind. Percentage Frequency from the following Directions. N.	N.E.	E.	SE.	S.	S.W.	W.	N.W.	Calm or Var.	Mean Barometric Pressure, 50 Years' Average.	Mean Temperature, 50 Years' Average. Maximum.	Minimum.	Daily Range.	Daily Variability.	Mean Temperature.	Charlotte Square Rainfall, 44 Years. *Percentage* Excess or Defect.
	Inches.	°	°	°	°	°	%	Ins.																	
January,	29·899	42·0	33·2	8·8	3·2	37·6	86·8	1·57	15	2	4	7	7	6	15	45	8	6	**·103**	*0·7*	**0·4**	*1·0*	*0·0*	*0·2*	*28*
February,	·882	44·5	34·4	10·1	2·9	39·4	86·2	1·63	15	3	5	10	9	5	15	46	8	9	*·033*	**0·2**	**0·6**	*0·4*	*0·0*	**0·4**	*3*
March,	·892	46·2	34·1	12·1	3·0	40·1	82·9	1·52	14	5	8	13	6	5	9	34	14	6	**·051**	*0·7*	*0·3*	*0·4*	**0·1**	*0·5*	*11*
April,	·904	51·4	37·4	14·0	2·5	44·4	80·2	1·50	14	5	13	27	9	5	8	19	8	6	**·017**	*0·6*	*0·2*	*0·4*	*0·2*	*0·4*	*2*
May,	·925	57·3	42·0	15·3	2·9	49·6	77·3	1·97	15	4	11	24	6	5	9	24	10	7	*·002*	*0·3*	*0·1*	*0·2*	**0·1**	*0·2*	**1**
June,	·959	62·6	47·6	15·0	2·6	55·1	75·8	1·92	13	5	12	24	4	4	8	27	9	7	**·028**	*1·0*	*0·2*	*0·8*	*0·2*	*0·6*	*10*
July,	·861	64·8	50·8	14·0	2·4	57·8	79·1	3·01	18	3	8	19	2	4	10	38	8	8	*·011*	*0·8*	**0·2**	*1·0*	*0·1*	*0·3*	**10**
August,	·847	64·5	50·5	14·0	2·5	57·5	81·1	2·93	19	3	6	18	4	3	9	41	9	7	*·007*	*0·5*	**0·2**	*0·7*	*0·1*	*0·1*	*1*
September,	·910	60·4	47·2	13·2	2·4	53·8	82·6	2·12	16	4	5	15	4	5	13	38	8	8	**·027**	*0·6*	**0·2**	*0·8*	*0·2*	*0·2*	*16*
October,	·851	53·7	41·1	12·6	2·8	47·4	85·1	2·22	17	5	6	11	7	7	9	38	10	7	**·057**	**0·6**	*0·3*	**0·9**	*0·1*	**0·2**	*6*
November,	·828	46·8	37·0	9·8	3·1	41·9	86·5	2·16	17	5	4	9	5	6	19	34	11	7	**·023**	*0·0*	**0·5**	*0·5*	*0·0*	**0·3**	*11*
December,	·832	42·2	33·0	9·2	3·2	37·6	86·8	1·95	16	3	5	5	3	5	13	46	11	9	*·003*	*1·6*	*1·1*	*0·5*	*0·0*	*1·3*	*14*
Annual Means,	29·882	53·0	40·7	12·3	2·8	46·9	82·5	24·50	189	4	7	15	5	5	11	36	10	7	**·026**	*0·5*	*0·0*	*0·5*	*0·1*	*0·2*	*7*

* Rain at Charlotte Square. Rainy days at Newington.

On the Path of a Rotating Spherical Projectile. Part II. By Prof. Tait.

(*Abstract.*)

(Read January 6, 1896.)

In addition to the authorities quoted in the first part of the paper, memoirs by Clerk-Maxwell and by Lord Rayleigh are referred to, also a passage in the *Beiblätter zu den Ann. d. Physik.* (1895, p. 289), which cites Hélie, *Traité de Balistique.*

The Author then considers the more obvious defects of the rudely approximate solution of the differential equations which is given in Part I., especially the omission of the *direct* gravitational effect on the speed, and shows how to take account of the effect of the observed gradual diminution of the angular velocity of the projectile. An improved solution of the problem of § 8 of the paper is also given.

The Author stated that he had occasionally succeeded in obtaining the kink spoken of in Part I., the projectile being a humming-top made of very thin metal. He had also occasionally obtained a cusp, thus exhibiting the paradoxical result of a projectile's path which is at no point concave downwards.

(*Added Jan. 20th.*)

At this meeting the Author showed the kinked path to the Society in an exceedingly simple manner; the projectile being a very large and thin shell of india-rubber which, contrary to his expectations, was found to preserve for a considerable time the spin given to it by cutting it sharply, in an obliquely downward direction, with the flat hand. The operation is analogous to jerking in golf. This experiment can be successfully performed with great ease, and is a thoroughly illustrative one for the whole subject.

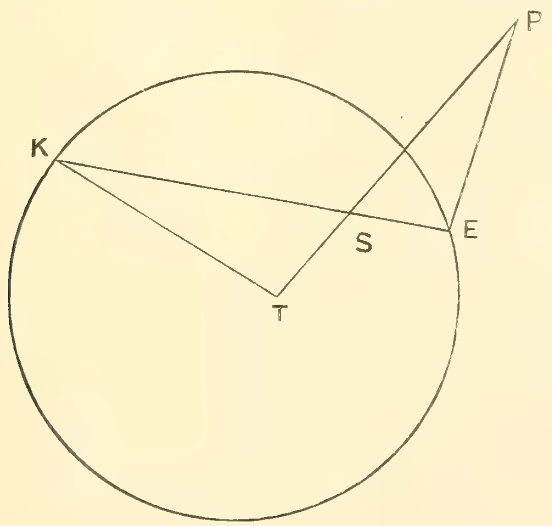
Note on Centrobaric Shells. By Prof. Tait.

(Read February 3, 1896.)

It is singular to observe the comparative ease with which elementary propositions in attraction can be proved by one of the obvious methods, while the proof by the other is tedious.

Thus nothing can be simpler than Newton's proof that a uniform spherical shell exerts no gravitating force on an internal particle. But, so far as I know, there is no such simple proof (of a *direct* character) that the potential is constant throughout the interior.

On the other hand the direct proof that a spherical shell, whose surface-density is inversely as the cube of the distance from an internal point, is centrobaric is neither short nor simple. (See, for instance, *Thomson and Tait's Elements of Natural Philosophy*, § 491.) But we may prove at once that its *potential* at external points is the same as if its mass were condensed at the internal point.



For if an elementary double cone, with its vertex at S, cut out areas K and E, we have

$$\frac{E}{SE^2} = \frac{K}{SK^2}.$$

Let P be any external point, and take T on PS (produced) so that

$$PS \cdot ST = KS \cdot SE = b^2.$$

Then we have obviously, from similar triangles,

$$SK \cdot EP = SP \cdot KT.$$

Thus
$$\frac{\left(\dfrac{E}{SE^3}\right)}{EP} = \frac{K}{SK \cdot SE \quad SK \cdot EP} = \frac{1}{b^2} \cdot \frac{1}{SP} \cdot \frac{K}{KT}.$$

But the sum of the values of $\dfrac{K}{KT}$ is the (constant) potential at T for unit surface-density; so that the sum of the values of the first side of the equation is inversely as SP; and the proposition is proved.

Although no mention has been made of Electric Images, in the above investigation, it is obvious that nearly all their chief elementary properties have been proved, almost intuitively, in the course of these three or four lines. The others are obtained at once by applying the same method to the case in which P is inside the spherical shell, and T outside:—remembering that the potential at T is now inversely as the distance of T from the centre, O, of the sphere; and referring the potential of E to a point S^1 on OS produced till $OS \cdot OS^1$ is the square of the radius of the shell.

On the Motion of a Heterogeneous Liquid, commencing from Rest with a given Motion of its Boundary. By Lord Kelvin, President.

(Read April 6, 1896.)

I use the word "liquid" for brevity to denote an incompressible fluid, viscid or inviscid, but inviscid unless the contrary is expressly stated. A finite portion of liquid, viscid or inviscid, being given at rest, within a bounding vessel of any shape, whether simply or multiply continuous; let any motion be *suddenly* produced in some part of the boundary, or throughout the boundary, subject only to the enforced condition of unchanging volume. Every particle of the liquid will instantaneously commence moving with the determinate velocity and in the determinate direction, such that the kinetic energy of the whole is less than that of any other motion which the liquid could have with the given motion of its boundary.* This proposition is true also for an incompressible elastic solid, manifestly; (and for the ideal "ether" of *Proc. R.S.E.*, March 7, 1890; and Art. xcix. vol. iii. of my Collected Mathematical and Physical Papers). The truth of the proposition for the case of a viscous liquid is very important in practical hydraulics. As an example of its application to inviscid and viscous fluid and to elastic solid consider an elastic jelly standing in an open rigid mould, and equal bulks of water and of an inviscid liquid in two vessels equal and similar to it. Give equal sudden motions to the three containing vessels: the instantaneous motions of the three contained substances will be the same. Take, as a

* *Cambridge and Dublin Mathematical Journal*, Feb. 1849. This is only a particular case of a general kinetic theorem for any material system whatever, communicated to the Royal Society, Edinburgh, April 6, 1863, without proof (*Proceedings*, 1862–63, p. 114), and proved in Thomson and Tait's *Natural Philosophy*, sec. 317, with several examples. Mutual forces between the containing vessel and the liquid or elastic solid, such as are called into play by viscosity, elasticity, hesivity (or resistance to sliding between solid and solid), cannot modify the conclusion, and do not enter into the equations used in the demonstration.

particular case, a figure of revolution with its axis vertical for the containing vessel and let the given motion be rotation round this axis suddenly commenced and afterwards maintained with uniform angular velocity. The initial kinetic energy will be zero for each of the three substances. The inviscid liquid will remain for ever at rest; the water will acquire motion according to the Fourier law of diffusion of which we know something for this case by observation of the result of giving an approximately uniform angular motion round the vertical axis to a cup of tea initially at rest. The jelly will acquire laminar wave motion proceeding inwards from the boundary. But in the present communication we confine our attention to the case of inviscid liquid.

The now well-known solution* of the minimum problem thus presented, when the bounding surface is simply continuous, is, simply: that the initial motion of the liquid is irrotational. That the *initial* motion *must be irrotational* † is indeed obvious, when we consider that the impulsive pressure by which any portion of the liquid is set in motion is everywhere perpendicular to the interface between it and the contiguous matter around it, and therefore the initial moment of momentum round any diameter of every spherical portion, large or small, is zero. But that irrotationality of the motion of every spherical portion of the liquid suffices to determine the motion within a simply continuous boundary having any stated motion, is not obvious without mathematical investigation.

Whether the boundary is simply continuous, or multiply continuous, irrotationality suffices to determine the motion produced, as we now suppose it to be produced, from rest by a given motion of the boundary.

Now in a homogeneous liquid acted on by no bodily force, or only by such force (gravity, for example) as could not move it when its boundary is fixed, the motion started from rest by any movement of the boundary remains always irrotational, as we know from elementary hydrokinetics. Hence, if at any time the boundary is suddenly or gradually brought to rest, the motion of

* Thomson and Tait's *Natural Philosophy*, sec. 312.

† That is to say, motion such that the moment of momentum of every spherical portion, large or small, is zero round every diameter.

every particle of the liquid is brought to rest at the same instant. But it is not so with a heterogeneous liquid. Of the following conclusions Nos. (1), (2), (3) need no proof. To prove No. (4) remark that as long as there is any motion of the heterogeneous liquid within the imperfectly elastic vessel the liquid must be losing energy; and the energy cannot become infinitely small with any finite spherical portion of the liquid homogeneous.

(1) The initial motion of a heterogeneous liquid is irrotational only at the first instant after being *quite suddenly* started from rest by motion of its boundary. Whatever motion be subsequently given to the boundary the motion of the liquid is never again irrotational. Hence

(2) If the boundary be suddenly brought to rest at any time, the liquid, unless homogeneous throughout, is not thereby brought to rest; and it would go on for ever with undiminished energy if the liquid were perfectly inviscid and the boundary absolutely fixed. The ultimate condition of the liquid, if there is no *positive* surface tension in the interfaces between heterogeneous portions, is an infinitely fine mixture of the heterogeneous parts.* And, if there were no gravity or other bodily force acting on the liquid, the density would ultimately become uniform throughout. Take, for example, a corked bottle half full of water or other liquid with air above it given at rest. Move the bottle and bring it to rest again: the liquid will remain shaking for some time. An ordinary non-scientific person will scarcely thank us for this result of our mathematical theory. But, when we tell him that if air and the liquid were both perfectly fluid (that is to say perfectly free from viscosity), the well-known shaking of the liquid surface would, after a little time, give rise to spherules tossed up from the main body of the liquid; and that the shaking of the liquid, left to itself in the bottle supposed perfectly rigid, will end in spindrift of spherules which would be infinitely fine if the capillary tension of the interface between

* *Popular Lectures and Addresses*, by Lord Kelvin, vol. i. pp. 19, 20, and 53, 54. See also *Philosophical Magazine*, 1887, second half-year: "On the formation of coreless vortices by the motion of a solid through an inviscid incompressible fluid"; "On the stability of steady and of periodic fluid motion"; "On maximum and minimum energy in vortex motion."

liquid and air were infinitely small, he may be incredulous unless he tends to have faith in all assertions made in the name of science.

(3) If the boundary is an enclosing vessel of any real material (and therefore neither perfectly rigid nor perfectly elastic), and if it is laid on a table and left to itself, under the influence of gravity, the liquid, supposed perfectly inviscid, will lose energy continually by generation of heat in the containing vessel, and will come asymptotically to rest in the configuration of stable equilibrium with surfaces of equal density horizontal and increasing density downwards.

(4) With other conditions as in (3), but no gravity, the ultimate configuration of rest will be infinitely fine mixture (probably, I think of equal density throughout). Consider, for example, two homogeneous liquids of different densities filling the closed vessel, or a single homogeneous liquid not filling it. As an illustration, take a bottle half full of water, and shake it violently. Observe how you get the whole bottle full of a mixture of fine bubbles of air, nearly homogeneous throughout. Think what the result would be if there were no gravity, and if the water and air were inviscid and the bottle shaken as gently as you please; and if there were perfect vacuum in place of the air; or, if for air were substituted any liquid of density different from that of water.

Note on Clerk-Maxwell's Law of Distribution of Velocity in a Group of Equal Colliding Spheres. By Prof. Tait.

(Read June 15, 1896.)

The sarcastic criticism which M. Bertrand (*Comptes Rendus*, May 4 and 18, 1896) again bestows on Clerk-Maxwell's earliest solution of the fundamental problem in the *Kinetic Theory of Gases*, together with Prof. Boltzmann's very different, but thoroughly depreciatory, remarks (*ib.*, May 26), have led me to reconsider this question, already discussed by me at some length before the Society. Both of these authorities declare Maxwell's investigation to be erroneous:—but, while Prof. Boltzmann allows his *result* to be correct, M. Bertrand goes further, and bluntly calls it absurd. He had, in his *Calcul des Probabilités*, (1888), already given Maxwell's proof as an example of illusory methods. I have the misfortune to agree with Maxwell, and to hold that his reasoning, though not by any means complete, is (like his result) correct. (*Trans. R.S.E.*, vol. xxxiii. pp. 66 and 252.)

I have not found anything in these communications of mine (so far at least as the present question is concerned) which I should desire to retract; but they can be considerably improved; and I think that, by the introduction of the *Döppler-* (properly the *Römer-*) principle, the true nature of a part of the argument can be made somewhat more immediately obvious. Also I will venture to express the hope that Prof. Boltzmann may at last recognise that I have, in this matter at least, *not* deserved the reproach of having reasoned in a circle.*

1. The following quotation from my first paper (in which I have italicized the greater part of one sentence) shows the general ground of my reasoning, which was expressly limited to a very numerous group of equal, perfectly hard, spherical particles.

"Very slight consideration is required to convince us that, unless

* *Phil. Mag.*, xxv. (1888), pp. 89, 177.

we suppose the spheres to collide with one another, it would be impossible to apply any species of finite reasoning to the ascertaining of their distribution at each instant, or the distribution of velocity among those of them which are for the time in any particular region of the containing vessel. But, when the idea of mutual collisions is introduced, *we have at once, in place of the hopelessly complex question of the behaviour of innumerable absolutely isolated individuals, the comparatively simple statistical question of the average behaviour of the various groups of a community.* This distinction is forcibly impressed even on the non-mathematical, by the extraordinary steadiness with which the numbers of such totally unpredictable, though not uncommon, phenomena as suicides, twin or triple births, dead letters, &c., in any populous country, are maintained year after year.

On those who are acquainted with the higher developments of the mathematical *Theory of Probabilities* the impression is still more forcible. Every one, therefore, who considers the subject from either of these points of view, must come to the conclusion that continued collisions among our set of elastic spheres will, *provided they are all equal*, produce a state of things in which the percentage of the whole which have, at each moment, any distinctive property must (after *many* collisions) tend towards a definite numerical value; from which it will never afterwards markedly depart."

"When [the final result, in which the distribution of velocity-components is the same for all directions] is arrived at, collisions will not, in the long run, tend to alter it. For then the uniformity of distribution of the spheres in space, and the symmetry of distribution of velocity among them, enable us (by the principle of averages) to dispense with the only limitation above imposed; viz., the parallelism of the lines of centres in the collisions considered."

2. Now, considering the 3.10^{20} absolutely equal particles in each cubic inch of a gas, where could we hope to find a more perfect example of such a community? Where a more apt subject for the application of the higher parts of the *Theory of Probabilities*? If we are ever to find an approach to statistical regularity, it is surely here, where all the most exacting demands of the mathematician are fully conceded.

Is it not obvious, at once, that such a group must present *at all times, and from all sides*, precisely the same features? In other words:—that the solution of the problem is UNIQUE. (This word practically contains the whole point of the question). If not, the higher part of the Theory of Probabilities (in which M. Bertrand himself is one of the prominent authorities) is a mere useless outcome of analytical dexterity; and even common-sense, with consistent experience to guide it, is of no value whatever.

A first consequence of this perfect community of interests is that (on the average, of course) the fraction of the whole particles, whose component speeds *in any assigned direction* lie between x and $x + \delta x$ is expressed by

$$f(x)\delta x$$

where f is a perfectly definite (and obviously *even*) function.

It is clear from this that the density of ends in the velocity space-diagram *depends on r only*; but we require further information before we can find *how*. (M. Bertrand seems to admit the first statement; but he insists that, otherwise, the solution is *wholly arbitrary*.)

3. [But, before seeking this, we may take another mode of viewing the situation:—as follows. It is, of course, nothing more than an *illustration* of the argument just given.

Suppose, merely for the purpose of examining the condition of the gas, and therefore without any inquiry into other physical possibilities, which have nothing to do with the argument:—

That (a) each particle of the group is self-luminous, and all give out, with equal intensity, light of one definite period. (To illustrate the remark just made, note that this luminosity is *not* attributed to collisions, nor to any assigned physical causes).

(b) The wave-length of light reaching the eye from a moving source is altered by an amount proportional to the speed with which its distance from the eye alters.

(c) The displacement of light by a grating on which it falls normally is proportional to the wave-length.

(d) An ideal grating may be assumed, of any requisite regularity and fineness; and, again for the sake of argument only, it may be supposed to act, however fine it be, in the same manner as do ordinary gratings.

These premised, the spectrum of the gas will be a band, whose visible breadth depends only on the fineness of the grating and the luminosity of a particle. But this band will present, *at all times and from all sides* of the group, exactly the same appearance.

Its brightness, therefore, at any given distance from its central line, will be constant. But this means that the fraction of the whole number of particles which have any given speed in the line of sight, *depends on that speed alone.* The utmost speed of a gaseous particle is exceedingly small compared with that of light, and the alteration of wave-length is not affected by the part of the motion of the luminous particle which is transverse to the line of sight.]

4. We have not yet exhausted the consequences of absolutely perfect (average) community. For *every* particle, in virtue of citizenship, has a right to, and obtains, its due quota of whatever is shared among the group. Its tenure of any one value of x ceases (usually in a most abrupt way) some 10^{10} times per second, but leaves it *absolutely free* to have, during each of these brief periods, any values of y and z which may fall to it. There are, in fact, definite specifications of x, y, z speeds; but they are distributed among the particles with absolute independence of one another, in a manner which is perpetually changing at an exceptionally rapid rate. And the entire independence of x, y, and z speeds is shown by the fact that, in a collision, there is a mere interchange of speeds along the line of centres at impact:—*whatever* be the speeds of the impinging particles in other directions.

Thus the assumption, which Maxwell allowed "might appear precarious" (it is carefully to be observed that he did *not* say it appeared so to himself) is fully justified. In any element of volume of the space diagram of velocities, the density is proportional to

$$f(x)\,f(y)\,f(z)$$

whatever rectangular axes be employed. This, of course, gives at once Maxwell's result, viz. :—

$$\left(\frac{h}{\pi}\right)^{\frac{3}{2}}\epsilon^{-h(x^2+y^2+z^2)}$$

To any one who is doubtful about the accuracy, or the cogency, of the reasoning just sketched, we may put the matter in another form. The solution, we saw, is *unique.* But this is obviously *a*

solution, for it is easy to see that *collisions do not alter it.** Therefore it is *the* solution.

5. M. Bertrand treats the above result of Clerk-Maxwell's to the following sweeping condemnation :—

"Il y aurait indulgence à reprocher à cette formule trop peu de rigueur : les habitudes de la Géométrie autorisent à la déclarer tout simplement absurde."

Comment on this would be superfluous.

But it is easy to see how M. Bertrand has been led into this position. The following is, according to his information, the problem as proposed, and solved, by Maxwell :—

"Les molécules d'une masse gazeuse, étant en nombre immense et considéré comme infini, sont animées de vitesses inconnues. On ne sait rien sur les conditions initiales et sur les actions perturbatrices qui s'exercent entre elles et sur elles.

Déterminer le rapport du nombre total des molécules au nombre des celles dont la vitesse est comprise entre des limites données. On n'admet rien de plus, sinon que, par l'absence de toute ordonnance régulière, tout est pareil dans toutes les directions."

No wonder M. Bertrand says that this reminds one of the question of finding the age of the captain from the size of his vessel !

The real cause for wonder is that M. Bertrand, who must be perfectly aware that strong common-sense was as prominent a characteristic of Maxwell's intellect as was brilliant, and often daring, originality, could believe him capable of propounding such manifest nonsense.

6. What Maxwell *did* propose, and solve, was a very different problem indeed. Here are his words (*Phil. Mag.* xix. (1860), p. 22) :—

"Prop. IV. To find the average number of particles whose velocities lie between given limits, *after a great number of collisions among a great number of equal particles.*"

He had already pointed out that the particles are regarded as spherical and perfectly elastic ; and that, though collisions are

* With this particular form of $f(x)$ not only is $f(x)f(y)f(z)$ an *Invariant* in the usual sense of being independent of the rectangular system of axes employed ; but *its separate factors are unaltered* by a collision if one of these axes be taken parallel to the line of centres at impact.

perpetually altering the velocity of each, the tendency is to some regular law of distribution of *vis viva* among the group. I am far from asserting that his paper (which, epoch-making as it was, is evidently a somewhat hasty and unmatured effort) is free from even large errors; but it certainly does not contain such palpable absurdities as those now laid to its charge.

M. Bertrand entirely ignores the fact that Maxwell was dealing with a "community." And his comment on Maxwell might justly be retorted on himself in a slightly altered form. For he asserts that the x, y, z speeds are not independent, which is virtually the equivalent of the statement that when the latitude of a ship at sea has been anyhow determined, its longitude is no longer wholly indeterminate!

[July 6, 1896. Prof. Boltzmann, to whom I sent a proof of the above, requests me to add, on his part, as follows:—

"I have given expression to my high respect for Maxwell in the Prefaces to the two Parts of my *Lectures on Maxwell's Theory of Electricity and Light*, and specially in the Motto to Part II. And, besides, I regard Maxwell's discovery of the Law of Distribution of Velocity as so important a service that, in comparison, the trifling mistakes which appear to me to occur in his first proof are not worthy of consideration. The letters which I wrote to M. Bertrand, who was good enough to communicate them to the French Academy, had thus by no means the object of expressing my concurrence in M. Bertrand's dissentient (*abfällig*) judgment of Maxwell's work on the Velocity-distribution-law. I wished rather to say that M. Bertrand was so much the less justified in this opinion because the one objection he was able to make had already been made by others, who agree in all essentials with Maxwell.]

On the Structure of *Actinotrocha* considered in relation to the suggested Chordate Affinities of *Phoronis*. By A. T. Masterman, B.A., Lecturer and Assistant Professor of Natural History in the University of St Andrews.

(Read June 15, 1896.)

Some little time ago a preliminary note upon *Phoronis* was read before the Royal Society, and therein it was stated that a study of the anatomy of this group had led one to believe that its nearest phyletic allies were to be found in the *Hemichorda*, a group comprising a few species with primitive chordate characters.

In attempting to class *Phoronis* with these, an important difficulty presented itself. It has been usual to define the *Chordata* as having three essential characters in common, either temporarily in ontogeny or permanently throughout life, *i.e.*, a dorsal nervous system, paired gill-slits, and a notochord.

With regard to these features, there can be no doubt that the nervous system of *Phoronis* is dorsal in position, and its arrangement is exactly comparable to that of *Balanoglossus*; but, on the other hand, there are no gill-slits in the adult *Phoronis*, and the notochord is as certainly absent.

In the case of the gill-slits, we find upon critical examination that they are extremely variable in extent of number and position in the different *Chordata*, and in one species, *Rhabdopleura*, they are, according to present knowledge, entirely absent. For these reasons it is well not to insist on the presence of gill-slits as an essential character. With respect to the notochord a different condition prevails—the hypoblastic origin and vacuolated character of this organ mark it out from all other skeletal structures, and it figures more or less prominently in all the *Chordata*. This fact, and the consideration that *Phoronis* has a degenerate sedentary habit, with a well-developed mesoblastic skeleton of chondroid tissue comparable to that of the Vertebrata, replacing, in them, the notochord, taken in conjunction with the remarkable anatomical resemblances to *Balanoglossus* and *Cephalodiscus*, seemed to justify

the remark—"I cannot help suspecting that subsequent investigations will reveal some trace of an organ homologous to the notochord in the *Actinotrocha* stage."

A preliminary examination of spirit specimens of *Actinotrocha* has justified these suspicions in such a remarkable degree that I here purpose giving a short account of the results.

Not only does *Actinotrocha* possess structures which are indubitably of the morphological value of a notochord, but it also presents other features which, I think, must settle the affinity of *Phoronis* to be with the *Hemichorda.* As is well known, *Actinotrocha* has a large overhanging pre-oral lobe and a somewhat elongated body. Behind the mouth is a double row of tentacles, which form an almost complete post-oral ring.

A partial ring of ciliated cells follows the edge of the pre-oral lobe, a post-oral ciliated band takes a sinuous course up and down the tentacles, and a third ciliated ring surrounds the anus at the posterior end of the body, usually called the peri-anal band.

Actinotrocha is thus very little modified from a type of larva with three ciliated rings, the pre-oral, the post-oral, and the peri-anal, which correspond, as will be later shown, to the three segments of the mesoderm.

At the front end of the body is a thickened plate of ectodermal nervous tissue, usually termed the apical plate. The coelome is divided into *five* pouches or cavities, one unpaired, filling up the cavity of the pre-oral lobe, two paired cavities lying immediately behind the mouth and produced into the tentacles, and two posterior cavities filling up the trunk.

At about the level of the mouth ventrally, and stretching backwards to meet the dorsal ectoderm just in front of the apical plate, is a mesentery limiting the pre-oral cavity posteriorly.

In the immediate neighbourhood of the œsophagus this mesentery envelopes the blind end of the dorsal blood-vessel, and its walls are glandular, thus forming an organ similar to that formed in *Balanoglossus.* I cannot, however, speak with absolute certainty with regard to this point, for there are other structures connected with the pre-oral lobe which require elucidation by examination of fresh specimens. The paired post-oral or *collar* pouches are separated from the trunk cavities by paired mesen-

teries running from the base of the most posterior tentacles upwards and inwards to meet the œsophagus, and by paired mesenteries from the pre-oral cavity running in front of the first anterior tentacle inwards and downwards to the œsophagus (fig. 2). Under the apical plate, the collar walls and the pre-oral mesodermic wall do not come in contact, but leave a haemocoele space.

The dorsal and ventral blood-vessels and the paired lateral vessels are also haemocoelic spaces between the gut-wall and the mesoderm of the collar and trunk.

Fig 1

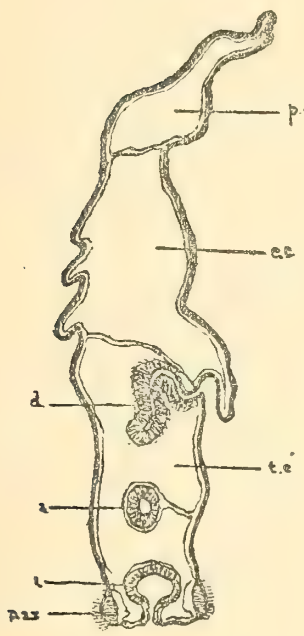


Fig. 1.—Longitudinal vertical section of *Actinotrocha,* to one side of median line.

Fig 2.

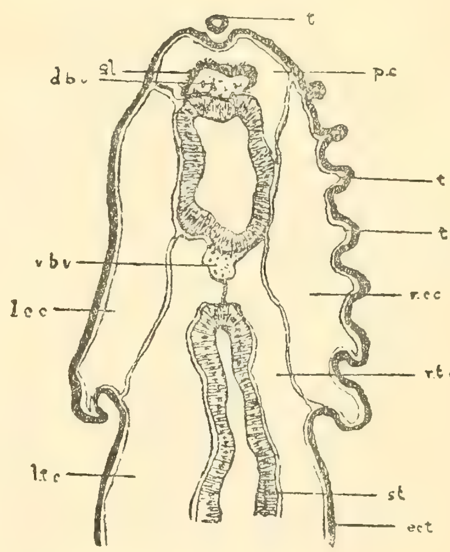


Fig. 2.—Longitudinal horizontal section (nearly) of *Actinotrocha.*

Lastly, a similar haemocoelic space forms a ring sinus just below the peri-anal ciliated band (fig. 1). There is one pair of nephridia which consist of ciliated tubes opening to the exterior on the ventral side below the ring of tentacles. The tubes run between the collar and trunk walls, and protrude into the collar cavities. They terminate in a branched internal funnel closely resembling that described for *Amphioxus* by Prof. Boveri. The apertures appear to open into the collar cavities, a fact which, if confirmed, would enable them to be homologised with the collar pores of *Balanoglossus.*

The tube and funnel are both surrounded by a thick mass of blood corpuscles.

As regards the organs of the ectoderm and mesoderm, *Actinotrocha* may be regarded as consisting of three segments, one preoral and two post-oral.

The organs of the endoderm have yet to be referred to.

The mouth leads by a short œsophagus into a wider stomach region, and the front wall of this stomach is produced forwards as a pair of hollow diverticula, lying laterally on each side of the

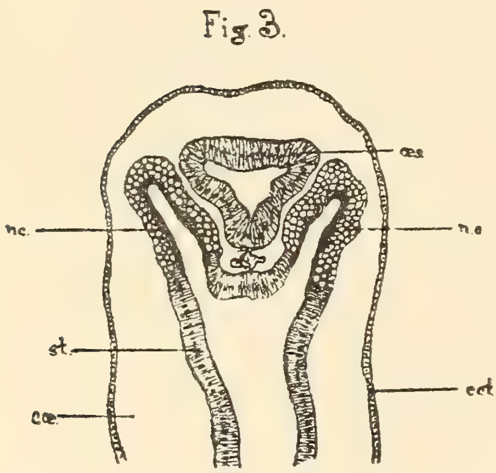


FIG 3.—Longitudinal horizontal section of *Actinotrocha,* in front of fig. 2.

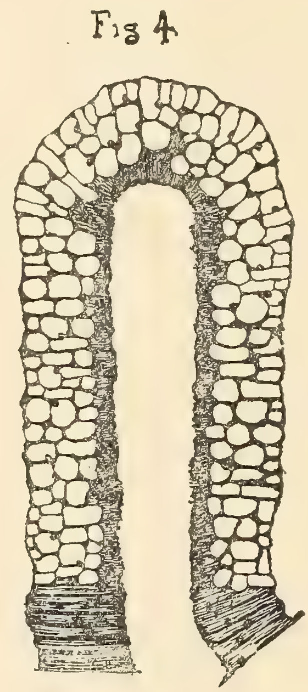


FIG. 4.—Notochordal diverticulum of *Actinotrocha,* as seen in fig. 3, but with high power.

œsophagus (fig. 3, *n-c.*). These two cœca arise from the region of the collar, that is, the part of the gut which is surrounded by the collar cavities, and they protrude into the collar space, almost coming into contact with the outer wall at the apex of the tentacular ring. The most superficial examination of sections shows that they cannot be regarded as glandular, but that the constituent cells have been metamorphosed into vacuolated tissue.* The process of vacuolisation proceeds from without inwards, and in the latest stage observed there is always a part of the wall lying *on the inside,* which had a number of nuclei lying in a mass of cell-protoplasm, in which the vacuoles are

* The vacuoles do not stain with borax-carmine or haematoxylin.

not found. The cœca remain in continuity with the wall of the gut, and retain their lumen.

An inspection of fig. 4 will show the close resemblance of these cœca to the typical notochordal tissue of the *Chordata.* They evidently lend support to the anterior region of the tentacular ring, and being thus sustentative organs, developed in response to the stimuli from outside themselves, the direction of the vacuolising process corresponds to that which would be expected on theoretical grounds.

I think there can be no reasonable doubt that these two hypoblastic vacuolated diverticula have the morphological value of a notochord.

Their origin in the collar region corresponds to that in *Balanoglossus,* the only hemichordate form the development of which is known, and the fact that they are paired and lateral instead of medium and dorsal need not be a serious impediment to the comparison here instituted; the difference between "paired" and unpaired is obviously one of degree rather than of kind in bilaterally symmetrical animals. On theoretical grounds one would expect a skeletal organ arising in the collar region to be paired.

It may be well not to press the immediate homology too far, especially as it is questionable if the precise homology of the notochord in such forms as *Balanoglossus, Tunicata, Amphioxus,* and *Vertebrata* can be maintained. It would be preferable to make a general definition of the notochordal feature, thus:—A skeletal structure formed by the vacuolisation of certain of the hypoblastic cells in definite areas determined by the special need in each group (in head, collar, or tail) which may (*Vertebrata*) or may not (*Hemichorda, Phoronis*) be separated completely from the gut wall, and may (higher *Vertebrata, Phoronis*) or may not (*Hemichorda, Cephalochorda*) be completely replaced in the adult by a mesoblastic chondroid skeleton.

The condition of the notochordal tissue in *Actinotrocha* is probably the most primitive yet described, for two reasons in particular; firstly, because the diverticulum still remains in organic continuity with the rest of the hypoblast, and its lumen in continuity with that of the gut; and secondly, because a certain area of the ventral wall of the gut just opposite the heavy sac-

like diverticulum, which is invaginated from the body wall, and later evaginated to form the greater part of the body of the adult *Phoronis*, is strengthened by a vacuolisation of certain of the hypoblast cells, which present a structure identical with that of the paired notochord in the collar region, but do not form a diverticulum.

They thus present the most primitive form of hypoblastic skeleton, the cells being metamorphosed *in situ*. The areas of notochordal tissue in *Actinotrocha* present a much more diffuse and *de facto* primitive condition than is found in the other *Chordata*. Their subsequent fate is unknown, but they are certainly not present in the adult.*

In the former paper was instituted a comparison between the structure of *Balanoglossus* and *Phoronis*, the points of similarity being well marked, provided due allowance be made for the different environment of these two animals and its effect in each case upon their anatomy.

As is to be expected, a comparison between the larval forms *Actinotrocha* and *Tornaria* reveals even closer harmony of structure; the resemblances between these two forms point to an extremely close affinity between them. It must be sufficient here to enumerate the leading characters in common:—

A large overhanging pre-oral lobe.

An apical plate, with, in some cases, two eyespots.

A pre-oral ciliated ring.

A post-oral ring, complicated (in some *Tornaria*) by tentacular protrusions of the body.

A peri-anal ring of cilia.

Gut with œsophagus, stomach, hind-gut, and terminal anus.

Mesoderm forming five cœlomic pouches, one pre-oral and two pairs post-oral.

Dorsal and ventral blood-vessels derived from the hæmocœlic space, connected by a collar-ring.

The only important character in *Tornaria* which, as far as is known, has no homologue in *Actinotrocha* is the proboscis pore.

Summing up, we may say that a study of the structure of

* I hope to shew later that there are paired lateral gut diverticula in the Asterid larva, which may be regarded as vestigial homologues of those here described.

Phoronis and of its larval form, *Actinotrocha*, clearly points to the following:—

Firstly, this genus is closely allied to the members of the *Hemichordata*, and *Balanoglossus* in particular.

Secondly, the present structure of *Phoronis* points to a marked degeneration, due to the assumption of a sedentary life, the degree of highest organisation being reached in *Actinotrocha*, just before the metamorphosis which results in the transformation to the adult condition.

Thirdly, the structure of *Actinotrocha* conforms to the hemichordate type extremely closely, even to minute particulars.

The evolutionary processes here indicated have an exact parallel in the relationship of the *Urochorda* (or *Tunicata*) to the *Cephalochorda*. In a precisely similar manner the tailed *Urochorda* larva reaches a certain maximum of structural complexity not far removed, if perhaps a little above, that of the adult *Amphioxus*, and then degenerates to the adult Ascidian, with a similar loss of preoral nervous system, sense organs, and notochord.

Perhaps the main difference in the two processes is this, that the larval Ascidian becomes fixed by the pre-oral lobe, as also in, for example, the *Echinodermata*; whereas the *Actinotrocha* becomes fixed by that organ which is probably homologous with the budding stolon of *Cephalodiscus*, and the permanently attaching cord of *Rhabdopleura*. This ontogenetic difference is also probably of phylogenetic value, and would largely explain the differing types of metamorphosis.

In conclusion, I would suggest that these relationships justify the classification of *Phoronis* as a separate division of the *Chordata*, which may be termed the *Diplochorda*, so that the *Chordata*, as thus constituted, would be as follows:—

Chordata—A. Trimetamera.

1. Diplochorda—*Phoronis*.
2. Hemichorda—*Balanoglossus*, *Cephalodiscus*, *Rhabdopleura*.

B. Polymetamera.

3. Urochorda—*Ascidia*, &c.
4. Cephalochorda—*Amphioxus*.
5. Holochorda—*Pisces*, *Amphibia*, &c.

It has of late become the custom, especially since the appearance of Professor Spengel's monograph on *Balanoglossus* and by certain German writers, to question the claims of this genus to vertebrate affinities. I would therefore point out that its close relationship to *Phoronis*, advocated in this and a former paper, is quite independent of the validity of these claims, though of course the right to consider *Phoronis* in any way connected with the vertebrate tree stands or falls with that of *Balanoglossus.*

REFERENCES IN WOODCUTS.

c.c.,	collar cavity.	*œs.*,	œsophagus.
cœ.,	cœlome.	*p.c.*,	pre-oral body cavity.
d.,	diverticulum or pouch.	*p.a.r.*,	peri-anal ring.
d.b.v.,	dorsal blood-vessel.	*r.c.c*,	right collar cavity.
ect.,	ectoderm.	*r.t.c.*,	right trunk cavity.
gl.,	pre-oral gland.	*st.*,	stomach.
i.,	intestine.	*t.*,	tentacle.
l.c.c.,	left collar cavity.	*t.c.*,	trunk cavity
l.t.c.,	left trunk cavity	*v.b.v.*,	ventral blood-vessel
nc.,	notochord.		

Some Results obtained with the Röntgen X-Rays.

By J. Macintyre, M.B., F.R.M.S.

(Read April 6, 1896.)

A preliminary demonstration on photography and fluorescence was given. A description accompanies the report on the communication made on 4th May 1896.

When Professor Röntgen's results were first made known the greatest attention was paid to the photography of objects which had previously been considered beyond the range of human vision. It is clear, however, that the discoverer did more than present us with a new photography by a force differing, in many respects, from day or artificial light. In his first paragraph he describes fluorescence on a prepared screen, and in the second he shows how one may *see shadows* of the bones within a faint dark shadow of the hand itself. The obvious advantage of being able to throw shadows of the deep-seated structures of the body upon screens cannot be over-estimated for medical and surgical purposes. The following experiments were undertaken with a view of contributing something to the work in this direction.

With regard to the salts employed, screens were prepared of potassium-platino-cyanide, barium-platino-cyanide, lithium-rubidium-platino-cyanide, calcium tungstate in its crystalline form (Edison), sulphide of calcium, magnesite, fluorspar, &c. In my experience it may be said at once that the potassium and barium-platino-cyanide salts were found to be the best, although the lithium-rubidium-platino-cyanide gave an excellent picture. The fluorescence of the potassium salt is probably in advance of all others, but occasionally one seems to get at least a more pleasant picture of the deep-seated structures of the body with the barium screen, particularly when the latter has a thick coating.

My earliest attempts failed on account of too little salt being used on the screen. In order to get a good sharp image, it is

essential that a thick coating should be spread upon the material forming the screen. This probably indicates that the phenomenon is not a surface one, but that the rays pass deeply into the salt. In my experience, however, it is better not to crush the crystals too much, although a certain amount of fine division must take place before it can be properly and evenly attached to the surface.

The salts may be placed on paper, probably black being better than ordinary paper; or again, on muslin screens, or thin wood or vulcanite. For practical purposes I find paper more convenient.

To obtain the best results, the Crooke's tube should be enclosed in black paper or cloth, so as to do away with the effect of the ordinary light in the tube. Moreover, the room should be thoroughly dark, and the spark of the interrupter of the coil should also be covered.

With regard to the use of the screen in a dark room, it may be here pointed out that some confusion exists, in the popular mind at least, owing to the description of Salvioni's cryptoscope. It need hardly be pointed out here that Röntgen himself not only photographed the shadows, but proved that images of deep-seated structures could be obtained by means of fluorescent screens. All that Salvioni has done is to arrange a small box so as to do away with the dark chamber, but it is not as efficient. Salvioni himself admits, in his original paper, that he has done nothing which could not be deduced from the original experiments.

The screens may be prepared in different ways, but I have found the following the best:—A quantity of barium-platino-cyanide is rubbed in a mortar until it forms a fine powder, and is then mixed with a solution of mucilage until a thickish yellow fluid is obtained of a consistency capable of being poured upon the paper, much as one would pour on collodion after the old wet plate photographic process. This when allowed to dry slowly gives satisfactory results. Another way is to stretch a piece of paper on a wooden frame, next to paint it over with a solution of gum, and through a metal sieve to dust on the powdered crystals of the salt to be employed. The coating should

be uniform, thick, and the distinctness of the image is somewhat increased by moistening the surface at the time of using. By means of these screens I have been able to see shadows of the bones of the extremities with their joints, the spinal column, ribs, sternum, clavicle, and face. The parts of the body which have offered the greatest resistance to the rays, and given the least satisfactory results in definition, are the cranium and pelvis. In these two cases the rays easily penetrated the parts, but definition was not quite satisfactory.

Additional Notes on the Röntgen X-Rays.

By Dr John Macintyre, Glasgow.

(Read May 4, 1896.)

The first series of photographs thrown on the screen included the different parts of the human skeleton. The second series illustrated photographs of the soft tissues, such as the tongue, larynx, heart, and diaphragm, all from the living subject. The third series were taken with a view to reduction of the time in exposure. With the fourth is given a description of some experiments upon attempts to polarise the x-rays.

The apparatus now most commonly employed was used in these experiments, viz., electricity (from the main), an induction coil giving a range of from 2 to 10 inch spark as required, a Crooke's tube having an aluminium cathodal disc, and a small plate of platinum for the positive pole. It is now known as the focus tube. Paget xxxxx-plates were used, as no particular advantage had been obtained from those specially prepared. The following points, amongst others, were found to be of great service in obtaining the best results:—Firstly, the current passing to the coil should be regulated according to the work to be done, and as little variation as possible should take place during the exposure. Primary batteries are not as satisfactory on account of this, and the best source is from the main. Lord Kelvin's cell tester and ampere gauge were used in the experiments, and any variations in the current were regulated by means of a rheostat. Secondly, the Apps or other coil or transformer must give, with the present form of tube, six inches or even more of spark when the deeper tissues of the body, such as the spine, are being taken. Thirdly, the vacuum of the tube should be carefully regulated and tested to give the maximum results. In the best tubes this was done before taking them off the pump, and the most satisfactory test was the fluorescent screen. During the exposure the vacuum changes from time to time. This is indicated by an alteration of

the fluorescence of the bulb by sparking outside of the tube across the terminals, and by different sounds produced by the interrupter. By gently heating with the spirit-lamp or Bunsen burner this difficulty disappears at once, and so a fairly constant source of x-rays may be maintained for a long period.

In taking photographs of the deeper structures of the body, the following points are worthy of attention:—Firstly, penetration; secondly, definition; thirdly, the photography of structures in the body which may have other structures placed in front or behind them, but which we do not desire to photograph: in other words, the selection of any particular structure within the body, and the omission of others. With regard to penetration, all the tissues of the body absorb the rays in a greater or less extent. While the bones absorb more than others, and therefore give the most striking pictures of the shadows, the soft tissues also absorb some of the rays, and by a careful arrangement these may be made to appear on a photographic plate or fluorescent screen. The mere penetration of tissue is not a difficult matter. With the apparatus above described, and a current giving 12 volts 17 amperes, and a selected Crooke's tube, the rays were found capable of penetrating a door 4 inches thick, and coated with lead colour on each side, and in sufficient quantity, after passing through the air for 14 feet, to cause fluorescence on a screen in the next room. Again, the rays are sufficiently powerful to pass through two human bodies, or through 18 inches thick of pine wood; and the spine, ribs, and other deeper structures of the body may easily be shown on fluorescent screens. The rays may be generated in such force as to pass through these bones, giving such slight contrast between them and the spaces around that the former disappear, and the whole screen becomes one fluorescent mass. It is obvious, therefore, that definition is of great importance. While it is true we have no method at present of focusing in the ordinary sense, that is no evidence of refraction nor true reflection, yet correct definition may be obtained in the following manner;—If a piece of white paper be laid on a table, and a pencil held at a short distance from it, a shadow of the pencil will be got on the paper. The nearer the pencil is to the paper the sharper the image will be. If the pencil be now removed from the paper towards the source of light, the

shadow will become less distinct, but if the pencil be held in that position, and the source of light removed still further from the pencil, the shadow becomes again distinct. So it is with the x-rays, and we can formulate a rule that there is a definite relationship between the position of the source of light, the object to be photographed, and the screen or sensitive plate upon which the shadow is to be thrown. The further the distance of the object to be photographed from the sensitive plate, the greater must be the distance between the object and the source of x-rays. In attempting, therefore, to photograph the deeper structures of the body or the tissues of the neck, it is evident we cannot get the object close to the sensitive plate, so the Crooke's tube must be removed a distance from it. No doubt this increases the time of exposure, but that will shortly be overcome with further improvements in the tubes. Where it is possible it is useful to test the result first on a fluorescent screen before photographing, just as a photographer would focus on a piece of ground glass before exposing.

The third point, viz.: How are we to photograph particular objects and omit others that may be lying in their vicinity? We can photograph straight through the human skull and omit one side of the head in the picture, and photograph the other although both are in the course of the x-rays, and between them and the sensitive plate. This is one of the many advantages of the tube above described. In this particular apparatus the cathodal torrent is led to the aluminium disc at one end of the tube, and is focussed on a small square of platinum directly in its course. The x-rays spring from this point, and radiate in every direction; in other words, form a cone, the apex being at the platinum plate. It naturally follows that if the x-rays be not proceeding on parallel lines, but diverging from a point, an object placed very near to the source of the x-rays will cast an indistinct image on the sensitive plate, but the structures which are near the plate wlll be distinctly brought out. Of course, were the bones of the head capable of arresting all the x-rays, nothing would be got, but it is only a matter of absorption in degree; so that, when we are photographing through the skull, sufficient rays pass through the one side to photograph the bone on the other, but the image on

that side next the tube is so diffuse that it never appears on the plate. By this means one can select different bones of the body, and so we can photograph the mastoid cells, or even show the meningeal grooves or sutures on the inside of the parietal and occipital bones themselves.

By carefully observing the above rules, and using an efficient apparatus, many of the soft tissues may easily be obtained. I have thus been able to photograph fasciæ, muscle, cartilage as well as bone, and on the screen you will see photographs of the larynx through the side of the neck, showing also the tongue, hyoid bone, cartilage of the organ; and in another picture the heart, showing the relationship to the ribs, the diaphragm below, and even a faint indication of the great vessels in the neck above.

The time of exposure varied. In the earliest experiments exposures of half an hour were required, but now many such photographs can be taken in the fraction of a second. Having noticed that the fluorescence of the tube was very much greater with a mercury interrupter than with the ordinary platinum spring, a series of experiments were carried out to determine the cause. It was noticed that although the current across the terminals, before passing to the coil, should reach as high as 17 amperes, during the action of the interrupter it fell to 1 ampere. On the other hand, with the mercury interrupter, and the same current during the exposure, it reached as high as 7 amperes. It was clear, therefore, that a current was passing through the primary coil by this arrangement which would mean more powerfully induced currents in the secondary coil, and consequently greater effect in the vacuum tube. By this system one could measure the exposure, not by seconds, but by the number of flashes in the tube corresponding to each interruption in the coil. With one flash, the current registering 10 volts and 10 amperes, and the coil giving 6-inch spark, it is possible with a Paget xxxxx-plate to get a photograph of the bones of the hand, and 10 such flashes give an excellent negative, showing the intimate structures. What the actual time represented in these exposures may be it is impossible to say; it must be an unknown fraction of a second. In any case, it is safe to say that the photograph is taken instantaneously.

Attempt to Polarise the Röntgen Rays.

Different views have been expressed about the possibility of polarising the x-rays by means of tourmalines; and although the following experiments seem to indicate a negative result, I take the liberty of placing them on record.

Apparatus employed.—The source of electricity was the main, and the measurements across the terminals, with Lord Kelvin's cell tester and ampere gauge, were 10 volts and 10 amperes. The spark of the coil was 6 inches, and a mercury interrupter was used. An ordinary Crooke's focus tube, enclosed in cardboard to exclude all light, was excited by the above, and the vacuum carefully arranged to give the maximum fluorescence by gently heating the bulb with a spirit-lamp. Screens of barium-platino-cyanide, potassium-platino-cyanide, and lithium-rubidium-platino-cyanide were tried. The two tourmalines were got as nearly alike as possible; the measurements of each were:—length, 47 mm.; breadth, 12 mm.; thickness, 2 mm.

First Observation.—On placing one tourmaline between the source of the x-rays and the screen, and directly in contact with the latter, a distinct shadow was seen, due to absorption of the rays. On placing the second tourmaline parallel with the first, a difference in density of the shadow was immediately observed. When the tourmalines were gradually turned at right angles to each other, a dark square area could be seen where the two crossed. A source of error was, however, suggested in this experiment. One of the tourmalines could not be in as close contact with the screen as the other, and, on account of the manner in which the x-rays pass from a point on the platinum plate in such a Crooke's tube, differences were observed in the shadows of the four arms of the cross formed by the tourmalines. (1) For example, if the horizontal tourmaline were next to the screen, and the vertical one behind it, the two arms above and below the square dark central area were less sharply defined than the two arms on each side of it, and consequently the shadows appeared to be different. (2) Although on the square portion corresponding to where the tourmalines

crossed we got a darker shadow still, it might only be due to the difference in thickness of the two layers.

Second Observation.—One of the tourmalines was broken in two portions, and one of these was placed parallel with, and the other perpendicular to, the other tourmaline. Again, the dark square area was seen by direct vision. I could not say, however, that the density was greater than where the other portion of the broken tourmaline was lying parallel with the whole one. This rather suggested that the square dark area was caused by difference of density only.

Third Observations.—These were made by photographs taken with different exposures. One with a single flash of the tube, due to one interruption of the coil; others were taken with much longer exposures, and in all the same difficulties in distinguishing between the two conditions arose. In the first photograph a shadow of one tourmaline is seen, proving the absorption of some of the x-rays. In the second, of one whole tourmaline, and a portion of the other, a greater density is to be noted where two layers are lying parallel with each other than where only one tourmaline interferes with the rays. The third photograph shows the whole tourmaline covered at one part by a portion of the broken tourmaline lying parallel with its axis. The other broken tourmaline is placed at right angles, and the question arises whether the density of the square area is greater than where the two tourmalines are lying parallel with each other. The photographs bear out the observations by direct vision, and would appear to give negative results.

On the Relation between the Hall Effect and Thermo-electricity in Bismuth and in various Alloys. By Dr J. C. Beattie.

(Read July 6, 1896.)

I have, in a previous communication* to the Society, given results for a number of alloys; these I propose to discuss from the point of view suggested by the title of this paper; I shall also discuss the results published by other experimenters, which relate chiefly to the behaviour of the Hall effect by constant field and variable temperature.

In attempting to explain the transverse effect, we have two things to consider. (1) The direction, and (2) the magnitude. In the following table the metals are arranged in a thermo-electric series as given by Wiedemann;† and in the second column the direction and magnitude of the transverse effect in terms of the rotatory coefficient R, according to Ettingshausen ‡ and Nernst's results, are given.

Metal.		R.
Bismuth, . .	−	10·1
Cobalt, . .	+	0·00459
Nickel, . .	−	0·0242
German silver, .	−	0·00053
Palladium, .	−	0·00115
Aluminium, .	−	0·00038
Lead, . .	+	0·00009 (or zero according to Hall)
Tin, . . .	−	0·00004
Copper, . .	−	0·00052
Platinum, . .	−	0·00024

* *Proc. Roy. Soc. of Edin.*, 1895.

† Wiedemann's *Lehre von der Electricität*, Bd. ii.

‡ *Sitzungsberichte der kaiserlichen Akademie zu Wien*, 1886.

Metal.		R.
Gold, . .	−	0·00071
Silver, . .	−	0·00083
Zinc, . .	+	0·00041
Cadmium, . .	+	0·00055
Iron, . .	+	0·0113
Antimony, .	+	0·192
Tellurium, .	+	532·000

The two irregularities, so far as direction is concerned, are cobalt and lead.

In considering the magnitude of the Hall effect, we must remember that different specimens have been used for thermo-electric and for transverse effect purposes; that slight changes in structure and in composition have a great influence on the thermo-electric position of a metal; and that probably the quantitative differences in the transverse effect in different specimens of the same metal are largely due to difference of structure and to the presence of foreign matter. Thus the irregularities in the second column in those metals which have a small transverse effect need not surprise us. The three metals in which the effect is greatest are bismuth, where the greatest negative effect is found, antimony, and tellurium; in the latter two the effect is positive, and in tellurium it is extremely great. Bismuth is at one end of the thermo-electric series, antimony and tellurium at the other.

We see that metals with an extreme position thermo-electrically have a correspondingly extreme position as regards the transverse effect. Without at present trying to explain what the exact relation between these two effects is, let us consider how we can experimentally test whether or not the above general conclusion is justified in other cases.

To do this we must examine other conductors, whose thermo-electric positions are at the extreme end of the series. Reference to a number of such, alloys and various minerals, will be found in the chapter on thermo-electricity in Wiedemann's *Lehre von der Electricität.* In the paper before referred to, the experimental arrangements and results with a number of alloys will be found. For the present purpose it is sufficient to give the table in which the results for the whole are gathered together.

No. of Plate.	Thermo-electric Series.	Sign of trans. effect in comp. metals.	Rotatory coefficient, R, for alloy for field 5610.	R from composition.
I.	19·5 Bis 1 Anti . .	− +	−11·65	−2·67
II.	10 Bis 1 Anti . .	− +	− 5·67	−2·56
III.	4 Bis 1 Anti . .	− +	− 5·26	−2·27
B.	Bis (pure) . . .	−	− 2·80	−2·8
IV.	2 Bis 1 Anti . .	− +	− 1·36	−1·92
V.	19·5 Bis 1 Lead . .	−0	− 0·155	−2·66
VI.	9·8 Bis 1 Lead . .	−0	0	−2·54
VII.	4·9 Bis 1 Lead . .	−0	+ 0·136	−2·35
	Anti	+	+ 0·191	
	Anti	+	+ 0·174	+0·174
VIII.	6 Anti 1 Zn . .	+ +	+ 0·144	+0·13006
XII.	806 Anti 406 Zn 121 Bis	+ + −	+ 1·44	−0·1385
IX.	806 Anti 406 Zn . .	+ +	+ 1·06	+0·1161
XIV.	806 Anti 696 Cd 406 Zn	+ + +	+ 3·96	+0·07357
XIII.	806 Anti 696 Cd 150 Bis	+ + −	+ 1·18	−0·16971
X.	1 Anti 1 Cd . .	+ +	+ 1·60	+0·08727
XI.	806 Anti 696 Cd . .	+ +	+ 6·06	+0·09308

The plates are numbered as in the previous communication.

In the second column the alloys and their composition by weight are given.

In the third the signs of the transverse effect in the component metals are given.

In the fourth the values of R, calculated from the transverse effect observed for a given field strength, are found.

In the fifth are given the values of R on the assumption that the transverse effect in the alloy can be calculated from the values in the component metals. The values for bismuth and antimony are taken from the results given in the table; for cadmium and zinc the values given by Ettingshausen and Nernst, given in the previous table, were used.

This last assumption has no application, it will be seen. In Plates I., II., III., VIII., IX., X., XI., XII., XIII., the actual values of R have no relation whatever to those obtained from it. The results given in the above table are all for the same field strength: it is well known, however, that the value of R for bismuth decreases as the field increases: this was found to be the case also with the bismuth-antimony and bismuth-lead alloys: in the others only a small decrease was observed between fields 5000 and 11,000; after that R is constant. Thus, for antimony and Plates

VIII., IX., X., XI., XII., XII., the R given in column 4 may be taken as holding for all field strengths, for bismuth and the other alloys this is not the case, R given can only be used for the field given, for the other fields its value is quite different, being larger for weak fields, smaller for strong ones; and in the bismuth-lead alloys it has a different sign even, according as the field is weak or strong. Taking all these things into consideration, it still remains true that the rotatory coefficient of an alloy cannot be calculated from those of its component metals.

On the other hand, a glance at column 4 shows us that a conductor, with an extreme thermo-electric position, has a large transverse effect. So far as the direction of the effect is concerned, we have here no exceptions. Thus, in Plates XII., XIII., instead of a negative effect due to the presence of bismuth, we have, in both cases, large positive effects comparable in magnitude with that of pure bismuth. With regard to the magnitude, the thermo-electric position of the Plates I., II., III. explain why it is we have in them such a large negative effect. At the other end of the series beyond antimony, we have, in Plates IX., X., XI., XII., XIII., positive effects greater than in antimony, and in numerical value of the same order of magnitude as in bismuth.

Here, however, there are irregularities. In Plates VIII., IX., X. the effect is smaller than we might expect, and in XIV. it is larger.

Leduc has also observed that the transverse effect in an alloy of equal parts of bismuth and lead is less than in silver. The position of this alloy in the thermo-electric series is next silver.

Hall also has observed that in alloys of copper and zinc the transverse effect is nearer that of copper than the composition of the alloy would lead us to expect. The thermo-electric position of these alloys is also nearer copper than the composition would lead us to expect.

To determine in which direction experimental effort should next be directed for the purpose of examining whether a relation between thermo-electricity and the transverse effect exists, we must remember that the latter is a function of the magnetisation and of the temperature. So far we have considered only cases where the temperature was approximately constant and the field variable;

that is, we have the transverse effect as function of the magnetisation alone. We must also consider the case where the field is kept constant and the temperature is varied: we then get the transverse effect as a function of the temperature, and we can compare it with the thermo-electric force of the couple formed by the conductor considered with lead when one junction is kept at a constant temperature, say zero centigrade, and the other is given successively the same temperatures as those at which the transverse effect by constant field was observed.

In the paramagnetic metals, iron, nickel, cobalt, the magnetisation is also a function of the temperature; this would have to be taken into consideration in experiments with these metals; in them the curve giving the variation of transverse effect with temperature would combine the peculiarities of the curves giving the variation of the magnetisation and of the thermo-electric force with temperature.

The only direct experiments made to determine whether or not a relation exists between the two effects are by Ettingshausen. He did not succeed in establishing any direct relation. This was to be expected, since, in the material he used, the transverse effect is really made up of two effects.

Perhaps the best metal in which to compare the two effects is bismuth. We have results for different specimens of this metal. Lebret* has examined the transverse effect at constant field strength, through a long range of temperature, in practically pure bismuth. Dewar and Fleming † have examined the thermo-electric force of bismuth-lead couples, in which one junction was kept at 0° C. and the other given temperatures from 180° C. to 100° C.

In the following tables the results for two specimens of bismuth, from the papers quoted above, are given. The transverse results are relative; the thermo-electric absolute.

* Communications from Physics Lab., Leiden, No. 19.

† *Philosophical Magazine*, 1895.

PLATE I.—*Magnetic Field about* 3000 *c.g.s. units.*

Plate I.		Plate II.	
Temperature in deg. C.	Hall effect proportional to	Temperature in deg. C.	Hall effect proportional to
− 74	1·256	− 69	0·866
− 72	1·250	− 64	0·889
− 65·5	1·245	− 59	0·915
− 58·5	1·246	− 51	0·970
− 52·5	1·244	− 47	0·985
− 47·5	1·241	− 41·5	1·007
− 40·5	1·211	− 35·5	1·016
− 36	1·211	− 31·5	1·030
− 27·5	1·190	− 25	1·032
− 23·0	1·180	− 19·5	1·034
− 14	1·139	− 12·0	1·040
− 7·5	1·110	− 8	1·034
0	1·056	− 4	1·034
+ 7	1·050	+ 0·5	1·034
+ 11	1·046	+ 7·5	1·034
+ 21	1·000	+ 21·5	1·028
+ 43·5	0·927	+ 26·5	0·980
+ 76	0·784	+ 56·5	0·869
+ 93	0·700	+ 84·5	0·725
+ 95	0·697	+105·0	0·634
+124	0·532	+124·5	0·540
+157·5	0·411	+152·0	0·729
+198·5	0·313	+203	0·288
+246·5	0·280	+219·0	0·217
		+242·5	0·175

The numbers given for Hall effect are expressed in terms of the effect for the plate considered at 21° C.

The general features of the two pairs of curves are almost exactly similar. In Lebret's plate I., there is a maximum at about − 74° C., and in his second plate a maximum at about − 20° C. On the other hand, in Dewar and Fleming's two specimens of commercial bismuth the curve of thermo-electric force has for plate 1 a maximum value at about − 43° C., for plate 2 a maximum at about − 81° C.

If we denote the numerical value of the transverse effect by E: then E can be expressed as a parabolic function of the temperature; using Lebret's figures we can for Plate II. put

$$-\mathrm{E} = at + bt^2$$

where a and b are constants + 0·00859 and ± 0·0000179 respectively

Commercial Bismuth-Lead Couple, No. 1 *and No.* 2, *one junction at* 0° *C.; the other at pt.*°

No 1.		No. 2.	
EMF of couple in C.G.S units.	Temperature pt. ° in degrees of plat. thermometer.	EMF of couple in C.G.S. units.	Temperature pt.° in degrees of plat. thermometer.
−579100	+100·1	−664150	+100
−458330	+ 84·0	−533680	+ 83·0
−334700	+ 66·3	−388400	+ 63·2
−212460	+ 46·2	−287060	+ 48·8
−100400	+ 24·1	−136130	+ 25·9
−64720	+ 15·6	−80600	+ 15·6
−40720	+ 9·1	−45050	+ 9·1
+11870	− 7·0	+37150	− 8·2
+53090	− 28·5	+113070	− 28·4
+64580	− 44·4	+153140	− 43·0
+63600	− 52·1	+168800	− 51·0
+52480	− 64·2	+184340	− 61·6
+47810	− 70·8	+189780	− 68·9
+29640	− 87·9	+190120	− 81·0
+2700	−102·2	+200110	− 99·7
−35570	−119·5	+192340	−115·0
−70960	−135·2	+174830	−132·6
−105760	−153·3	+153910	−152·0
−130870	−165·3	+140310	−163·2
−159240	−178·7	+117460	−177·4
−167740	−186·9	+108890	−184·7
−187370	−195·5	+87350	−195·5

t is the absolute temperature; the formula holds throughout nearly the whole range of temperatures experimented with, viz., from −69° C. to +242·5° C. At the two highest temperatures, 219° C. and 242·5° C., there is a slight deviation from the parabolic form of the curve.

In Plate I., the values of a and b are 0·0126 and −0·000034 respectively. The formula holds from the lowest temperature at which the experiment was carried out, viz., −74° C., till about 24° C. After that the curve deviates altogether from the parabolic form.

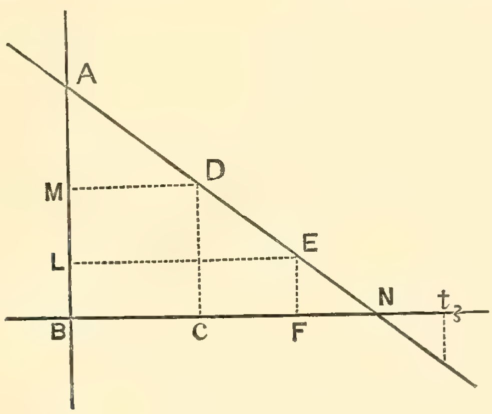
The fact that E can be represented by such a temperature curve suggests another way of expressing the results. For, consider the variation of E with the temperature, viz.,

$$\frac{dE}{dt} = a + 2bt$$

Mark off the values of $\frac{dE}{dt}$ along the perpendicular axis and t along the horizontal axis, we get then a straight line cutting the horizontal axis in the point $t = -\frac{a}{2b}$.

At this temperature we have the maximum transverse effect: thus for Plate I. the maximum is at $-23°$ C., and for Plate II. at $-73°$ C.—values which agree with the experimental results.

Let such a line be ADEN, then at C temperature t_1, the total transverse effect will be represented by the area ADCB; this we can resolve into two parts, one represented by BD, it is proportional to $\frac{dE}{dt}$ and the absolute temperature t_1, the other by ADM, which is proportional to the absolute temperature. Similarly, for



the temperature t_2 we have the total transverse electromotive force represented by FL + ELA; at N, on the other hand, we have no component proportional to the transverse power $\frac{dE}{dt}$, and the total transverse component is here a maximum; at t_3 the component proportional to $\frac{dE}{dt}$ has in the above figure a negative value, the total transverse effect has decreased and will continue to do so until a temperature is reached as far beyond N as N is distant from the origin B, supposing, that is, that E can be expressed by a parabolic curve up to that temperature.

In analogy with the notation of thermo-electricity, we may call $\frac{dE}{dt}$ the transverse power of the conductor; and we can speak of

the total transverse effect at a given temperature as made up—except at the neutral temperature $t = -\frac{a}{2b}$—of two parts: (1) The transverse Peltier effect proportional to the transverse power multiplied by the absolute temperature, and (2) the transverse Thomson effect proportional to the absolute temperature.

Leduc has also expressed the electromotive force of the transverse effect as a parabolic function of the temperature. He obtained the maximum value at 48·7° C.

Hall has examined the variation of transverse effect with temperature by constant external field in several substances, chiefly, however, in the magnetic metals.

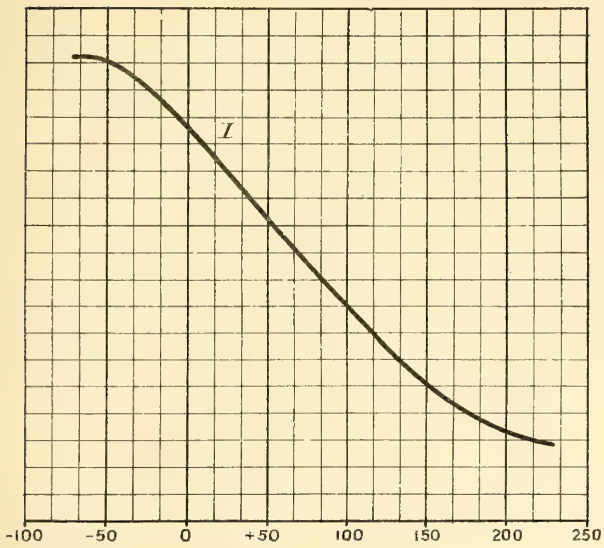
In them, as before stated, the effect must necessarily be a very complex affair; with nickel Hall has found that the curve representing the transverse effect at various temperatures bears a great resemblance to the corresponding magnetisation temperature curve. The effect of thermo-electricity, if it exists in this metal, is completely masked at the temperatures Hall has experimented at; what results would be given at lower temperatures, such as those used by Dewar and Fleming in their experiments on thermo-electricity, must be settled by new experiments. At any rate, it suggests a method for the consideration of the magnetic properties of the paramagnetic bodies at low temperatures.

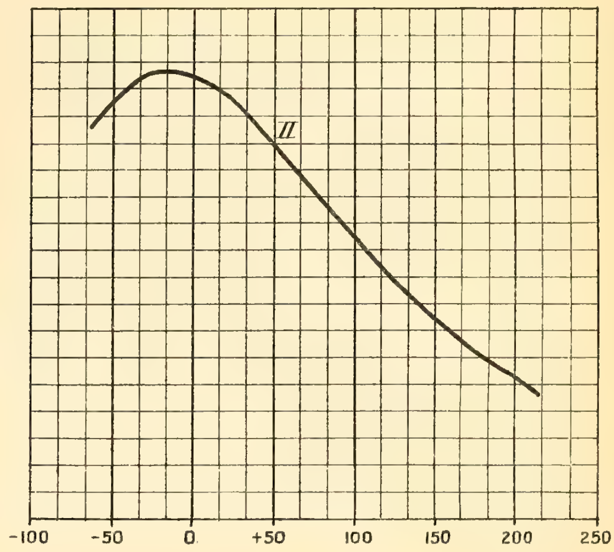
There are other substances which give better hopes of definite results than the paramagnetic metals. A glance at the curves published with Dewar and Fleming's above-cited article will show what these are. Antimony, for instance, has a very characteristic curve; the tangent to its thermo-electric force curve is twice parallel to the line of lead.

Palladium and platinum have also very distinctly marked maximum values.

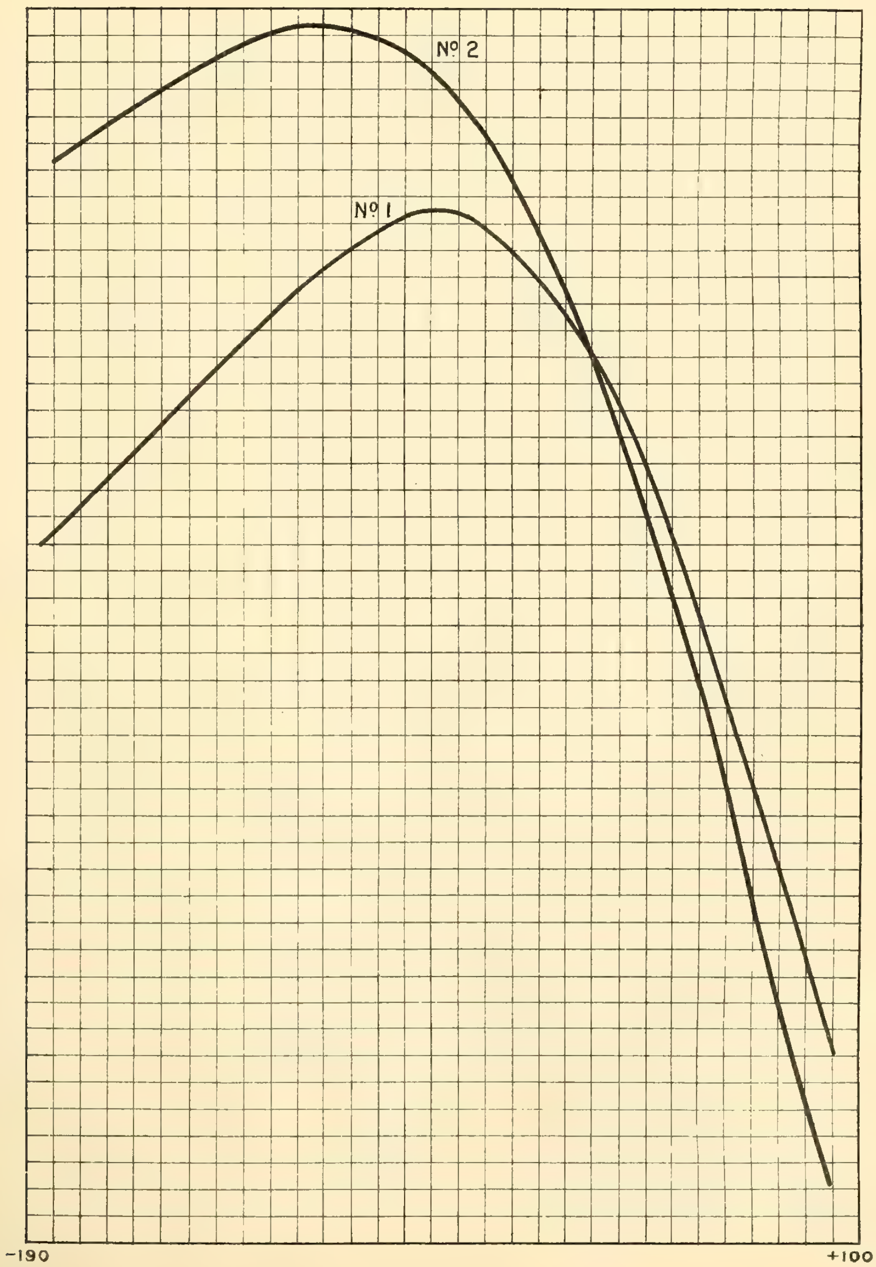
Evidently much more experimental data is necessary before we can say quite generally that the transverse effect is a function of the thermo-electric force; when we have these we may, perhaps, profitably study the exact relation between the molecular motion of heat and that of magnetism. At present we are only justified in saying that in bismuth and in alloys there is a very close connection between the thermo-electric properties and the transverse effect. Probably

CURVES SHOWING VARIATION OF HALL EFFECT WITH THE TEMPERATURE.





CURVES OF THERMOELECTRIC FORCE FOR BISMUTH-LEAD COUPLES.



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this connection will also be found in a number of other substances whose thermo-electric position is extreme. Selenium, for instance, which lies beyond tellurium, ought to have an extremely large positive transverse effect. Copper pyrites, arsenical pyrites, lead glance, iron glance, should have a large negative transverse effect; similarly, those sulphur compounds of the metals whose thermo-electric position is so peculiar should also have a large negative effect.

Preliminary Note on Dietthio and Diethylsulphone derivatives of Succinic Acid. By Professor Crum Brown and R. Fairbairn, B.Sc.

(Read July 20, 1896.)

I. Action of Sodium Mercaptide on Dibromosuccinic Ether.

Sodium mercaptide and dibromosuccinic ether, in the proportion of two molecules of the former to one of the latter, were dissolved separately in absolute alcohol, and slowly mixed. A considerable evolution of heat took place, while sodium bromide separated out. The flask was then digested for some hours on the steam-bath. The alcohol was subsequently distilled off, and the residue, on cooling, was treated with water. An oil separated out. This oil was collected by means of a separating funnel, and the aqueous layer several times extracted with ether. The oil and the ethereal extracts were added together and dried over calcium chloride. Next morning the ether was distilled off at the ordinary pressure. The remainder was distilled in vacuo. Between 50° and 60° a few drops came over, which proved to be ethyldisulphide.

The remainder came over between 150° and 170°.

This latter fraction was redistilled, and a portion of it used for analysis. The boiling point at 20 mm. pressure was 160°.

Combustion of dietthiosuccinic ether.

Weight of substance taken = ·2477 gram.
Weight of carbonic acid obtained = ·4476 gram.
Weight of water obtained = ·1702 gram.

Calculated for $C_{12}H_{22}S_2O_4$.	Found.
C = 49·0.	C = 49·3.
H = 7·5.	H = 7·6.

Determination of sulphur in dietthiosuccinic ether.

Weight of substance taken = ·1315 gram.

Weight of $BaSO_4$ obtained = ·1992 gram.

Calculated for $C_{12}H_{22}S_2O_4$.	Found.
S = 21·7.	S = 20·8.

Hence the reaction proceeded according to the following equation :—

$$\begin{array}{l} COOC_2H_5 \\ | \\ CHBr \\ | \\ CHBr \\ | \\ COOC_2H_5 \end{array} + 2C_2H_5SNa = \begin{array}{l} COOC_2H_5 \\ | \\ CHSC_2H_5 \\ | \\ CHSC_2H_5 \\ | \\ COOC_2H_5 \end{array} + 2NaBr.$$

II. Action of Baryta on Dietthiosuccinic Ether.

Dietthiosuccinic ether was boiled with the quantity of baryta required for barium dietthiosuccinate. When the solution became neutral, only about half of the ether was saponified. More baryta was added, and the solution was boiled till all the ether was saponified. Carbonic acid was passed through the cooled solution in order to convert the excess of baryta into barium carbonate. The gas evolved during the passage of the carbonic acid through the solution, was passed through a solution of mercuric chloride, when the white compound of mercuric chloride and mercaptan was formed.

The filtrate from the barium carbonate contained a very soluble barium salt, which did not crystallise on evaporation. It was, however, insoluble in alcohol. On adding alcohol to the aqueous solution, the barium salt was precipitated as a white amorphous substance.

Two barium estimations were made.

(I.) Weight of substance taken = ·3606 gram.
Weight of $BaSO_4$ obtained = ·2696 gram.
Percentage of barium = 44·0.

(II.) Weight of substance taken = ·2725 gram.
Weight of $BaSO_4$ obtained = ·2029 gram.
Percentage of barium = 43·8.

Percentage of barium in
$$\left.\begin{array}{l} COO \\ | \\ CH \\ \| \\ CSC_2H_5 \\ | \\ COO \end{array}\right\rangle Ba = 44{\cdot}0.$$

The barium salt was then converted into the acid by adding the calculated quantity of sulphuric acid. On evaporating the filtrate from the barium sulphate, the acid was obtained as a white solid, easily soluble in water, alcohol, and ether; but insoluble in ligroin and chloroform. The only insoluble salt was the lead salt. The acid gave on analysis the following numbers:—

Weight of substance taken	= ·0996 gram.
Weight of carbonic acid obtained	= ·1498 gram.
Weight of water obtained	= ·0413 gram.

Calculated for
$$\begin{array}{l} COOH \\ | \\ CH \\ \| \\ CSC_2H_5 \\ | \\ COOH \end{array}$$

Calculated.	Found.
C = 40·9	C = 40·9
H = 4·5	H = 4·6.

III. Action of Sodium Dibromosuccinate on Sodium Ethylsulphinate.

An aqueous solution of sodium ethylsulphinate was mixed with dibromosuccinic acid, which was nearly neutralised with sodium carbonate, and the mixture was heated for 8 hours at 140°. On cooling, long needles appeared in the tube. When opened, a violent escape of gas took place. The needles were placed on a filter, washed with water, and recrystallised from chloroform.

The substance gave on analysis the following numbers:—

Weight of substance taken	= ·1631 gram.
Weight of carbonic acid obtained	= ·1941 gram.
Weight of water obtained	= ·1015 gram.

Calculated for $C_6H_{14}S_2O_4$.	Found.
C = 33·6.	C = 32·5.
H = 6·5.	H = 6·9.

Determination of sulphur.

Weight of substance taken = ·1082 gram.
Weight of $BaSO_4$ obtained = ·2375 gram.

Calculated for $C_6H_{14}S_2O_4$.	Found.
S = 29·9.	S = 30·1.

The melting point was 138°. The melting point of ethylene diethylsulphone is 137°.

On the Linear and Vector Function. By Prof. Tait.

(Read May 18 and June 1, 1896.)

(Abstract.)

In the following Abstract I refer to such Linear and Vector Functions, only, as correspond to homogeneous strains which a piece of actual matter can undergo. There is no difficulty :—though caution is often called for :—in extending the propositions to cases which are not realizable in physics.

The inquiry arose from a desire to ascertain the exact nature of the strain when, though it is not pure, the roots of its cubic are all real :—*i.e.* when *three* lines of particles, not originally at right angles to one another, are left by it unchanged in direction.

1. The sum, and the product (or the quotient), of two linear and vector functions are also linear and vector functions. But, while the sum is always self-conjugate if the separate functions are so (or if they be conjugate to one another), the product (or quotient) is in general not self-conjugate :—though the determining cubic has, in this case, real roots. The proof can be given in many simple forms.

If ϖ and ω represent any two pure strains, there are three real values of g, each with its corresponding value of ρ, such that

$$\varpi\rho = g\omega\rho, \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

Assume $\omega^{\frac{1}{2}}\rho = \sigma$; and the equation becomes

$$\omega^{-\frac{1}{2}}\varpi\omega^{-\frac{1}{2}}\sigma = g\sigma.$$

But $\omega^{-\frac{1}{2}}\varpi\omega^{-\frac{1}{2}}$ is obviously self-conjugate. Hence the three values of g are real, and the vectors σ form a rectangular system. Thus (1) is satisfied by three expressions of the form

$$\rho = \omega^{-\frac{1}{2}}\sigma = g^{\frac{1}{2}}\varpi^{-\frac{1}{2}}\sigma \; ; \quad . \quad . \quad . \quad . \quad . \qquad (2).$$

i.e. there is one rectangular set of vectors which have their directions altered in the same way by the square roots of the inverses of each of the given strains.

But (1) may be written in the form

$$\omega^{-1}\varpi\rho = g\rho,$$

where $\omega^{-1}\varpi$ is in general not a self-conjugate function. Thus

Two pure strains in succession give a strain which is generally rotational, but whose cubic has three real roots.

Conversely, when a strain is such as to leave unchanged three directions in a body, it may be regarded as the resultant of two successive pure strains.

These are to be found from (2), in which the values of g and ρ are now regarded as *given*, so that the problem is reduced to finding ω (a *pure* strain), and the (rectangular) values of σ from three equations of the form

$$\omega^{\frac{1}{2}}\rho = \sigma.$$

When ω is thus found, the value of ϖ is given by (1). The solution is easily seen to express the fact that ω and ϖ, alike, convert the system ρ_1, ρ_2, ρ_3 into vectors parallel to $V\rho_2\rho_3$, $V\rho_3\rho_1$, $V\rho_1\rho_2$, respectively.

2. Other modes of solution of (1) are detailed, of which we need here mention only that which depends upon the formation of the cubic in

$$\phi = \varpi - g\omega,$$

the calculation of the coefficients in M_g, and the comparison of these forms with their equals found from

$$\phi = \varpi\omega^{-1} - g,$$

and from $$\phi = \omega^{-\frac{1}{2}}\varpi\omega^{-\frac{1}{2}} - g\,;$$

a process which gives interesting quaternion transformations.

3. Some curious consequences can be deduced from these formulae, which have useful bearing upon the usual matrix mode of treating the problem algebraically.

For, if we take

$$\varpi = \begin{pmatrix} A & c & b \\ c & B & a \\ b & a & C \end{pmatrix} \text{ and } \omega = \begin{pmatrix} p & 0 & 0 \\ 0 & q & 0 \\ 0 & 0 & r \end{pmatrix}$$

which involve complete generality since i, j, k are undefined, we have for the cubic (1) in g

$$\begin{vmatrix} \mathrm{A}-pg & c & b \\ c & \mathrm{B}-qg & a \\ b & a & \mathrm{C}-rg \end{vmatrix}=0.$$

The transformation of (1) given above is equivalent to dividing the successive rows, and also the columns, of this determinant by $\sqrt{p}, \sqrt{q}, \sqrt{r}$ respectively. It thus becomes

$$\begin{vmatrix} \mathrm{A}/p-g & c/\sqrt{pq} & b/\sqrt{pr} \\ c/\sqrt{pq} & \mathrm{B}/q-g & a/\sqrt{qr} \\ b/\sqrt{pr} & a/\sqrt{qr} & \mathrm{C}/r-g \end{vmatrix}=0,$$

from the form of which the reality of the roots is obvious.

A somewhat similar process shows that the roots of

$$\begin{vmatrix} \mathrm{A}-x & b & c \\ d & \mathrm{E}-x & f \\ g & h & \mathrm{I}-x \end{vmatrix}=0$$

are always all real, provided the single condition,

$$cdh=bfg,$$

be satisfied.

It is easy to see that this statement may be put in the form :— The roots of $\mathrm{M}_g=0$ are real, provided a rectangular system can be found such that

$$\mathrm{S}i\phi j\mathrm{S}j\phi k\mathrm{S}k\phi i=\mathrm{S}k\phi j\mathrm{S}j\phi i\mathrm{S}i\phi k.$$

The quaternion form, of which this is an exceedingly particular case, expresses simply that the roots of the cubic in ϕ are all real, if a self-conjugate function ω can be found, such that $\omega\phi$ is self-conjugate. This is merely another way of stating the chief result of § (1) above. But it may be interesting to illustrate it from this point of view. We may write, in consequence of what has just been said,

$$\mathrm{S}.\rho_1\rho_2\rho_3\ \phi\rho=g_1\mathrm{V}\rho_2\rho_3\mathrm{S}\rho_1\rho+g_2\mathrm{V}\rho_3\rho_1\mathrm{S}\rho_2\rho+g_3\mathrm{V}\rho_1\rho_2\mathrm{S}\rho_3\rho,$$

and

$$\omega\sigma=p_1\rho_1\mathrm{S}\rho_1\sigma+p_2\rho_2\mathrm{S}\rho_2\sigma+p_3\rho_3\mathrm{S}\rho_3\sigma.$$

These give at once

$$\omega\phi\rho = p_1 g_1 \rho_1 S\rho_1\rho + p_2 g_2 \rho_2 S\rho_2\rho + p_3 g_3 \rho_3 S\rho_3\rho$$

which is obviously self-conjugate.

4. The results above have immediate application to fluid motion. For, when there is a velocity-potential, the motion is "differentially irrotational":—*i.e.*, the instantaneous change of form of any fluid element is a pure strain; a particular cubical element at each point becoming brick-shaped without change of direction of its edges. But if we think of the result of two successive instantaneous changes of this character, we see that there is in general at every point a definite elementary parallelepiped, the lengths, only, of whose edges are changed by this complex strain. In special cases, only, is a similar result produced by three successive pure strains.

In connection with this Abstract I have now printed a little paper (read to the Society some time ago along with a speculation as to the *Antecedents of Clerk-Maxwell's Equations*), which deals with closely connected matters. It was again presented as illustrating some portions of my reply to Professor Cayley's paper on *Coördinates versus Quaternions* (*Proc. R.S.E.*, 2/7/94); but these portions were not printed at the time, because of a letter from Professor Cayley to the following effect:—

"I venture to suggest the omission of the passages relating to Heaviside—merely on the ground that it is making the question into a triangular duel. As far as I am concerned, it is certainly 'Quaternions,' and not 'Anything else', *v.* Coördinates, which I was arguing about."

The passage of my reply, which was suppressed in consequence of this request (though it formed a by no means unimportant part of my case), followed the first paragraph on p. 284 of the *Proc. R.S.E.* for July 2, 1894, and ran thus:—

"But I may refer to the recorded experience of a prominent practical worker at electro-magnetic theory, Dr O. Heaviside. His testimony is specially valuable in the present question because he is avowedly *not* a partizan of quaternions. His case may be described, in a modification of Prof. Cayley's Title, as "*Anything*

versus Coördinates." In the *Phil. Mag.* (June 1885) he has a paper *On the Electro-magnetic Wave-Surface,* from which I quote as follows:—

"Owing to the extraordinary complexity of the investigation when written out in Cartesian form (which I began doing, but gave up aghast) some abbreviated method of expression becomes desirable, I may also add, nearly indispensable, owing to the great difficulty in making out the meaning and mutual connections of very complex formulæ. In fact, the transition from the velocity-equation to the wave-surface by proper elimination would, I think, baffle any ordinary algebraist, unassisted by some higher method, or at any rate by some kind of short-hand algebra."

Dr Heaviside then considers the fitness of quaternions to supply the want, and refers unfavourably to my treatment of Fresnel's wave-surface. (Hamilton, in his *Elements,* highly commended even the first (very imperfect) form of it!) Next, Dr Heaviside develops a vector-system of his own, and with considerable labour and at comparatively great length obtains results which (as rendered for me, from his Volapuk into Quaternions, by Dr Knott) are found to be precisely what Quaternions directly and naturally give in a few lines! Dr Heaviside, in a semi-defiant attitude, gives away his case by asserting that his process "cannot be made more direct, or shorter, except of course by omission of steps, which is not a real shortening". Hence, and also of course, the Quaternion wins, hands down, against his new system. This particular application (including the simple proof of the identity of the various forms of the equations, which seems to have given Dr Heaviside very serious trouble) was made (partly by Hamilton, partly by myself) in 1859, and is to be found in §§ 382, 385 of the first edition of my *Quaternions,* or §§ 182, 439 of the third edition. The change of a few letters, to introduce the slightly wider data of the new problem, is all that is required."

On the Electro-magnetic Wave-Surface. By Prof. Tait.

(Read April 2, 1894.)

We may write the electro-magnetic equations of Clerk-Maxwell as

$$\phi\dot{\theta}_1 = \mathrm{V}\nabla\theta_2\ ,\quad \psi\dot{\theta}_2 = -\mathrm{V}\nabla\theta_1$$

For plane waves, running with normal velocity $v\alpha = -\mu^{-1}$, we have

$$\theta_1 = \epsilon f(vt + \mathrm{S}\alpha\rho)\,,\quad \theta_2 = \eta f(vt + \mathrm{S}\alpha\rho)$$

whence at once

$$\phi\epsilon = \mathrm{V}\mu\eta\,,\quad \psi\eta = -\mathrm{V}\mu\epsilon\,,$$

$$\text{so that } \mathrm{S}\mu\phi\epsilon = 0\,,\ \mathrm{S}\mu\psi\eta = 0\,.$$

[For the moment, we assume that ϕ and ψ are self-conjugate, so that a linear function of them is also self-conjugate. And we employ the method sketched in Tait's *Quaternions*, §§ 438–9.]

We have $$n\psi^{-1}\phi\epsilon = \mathrm{V}\psi\mu\psi\eta = -\mathrm{V}.\psi\mu\mathrm{V}\mu\epsilon$$

or $$\psi\mu\mathrm{S}\epsilon\psi\mu = n\phi\epsilon + \mathrm{S}\mu\psi\mu\,.\,\psi\epsilon = \varpi\epsilon \qquad \text{say.}$$

Thus we have, to determine μ, the single scalar equation

$$\mathrm{S}\,.\,\mu\phi\varpi^{-1}\psi\mu = \mathrm{S}\mu(n\psi^{-1} + \mathrm{S}\mu\psi\mu\,.\,\phi^{-1})^{-1}\mu = 0 \qquad (a)$$

This is the index-surface, and the form of ϖ shows that it has two sheets:—*i.e.*, there are two values of $\mathrm{T}\mu$ for each value of $\mathrm{U}\mu$.

The tangent plane to the wave is $\mathrm{S}\mu\rho = -1$ . . . (*b*)

To shorten our work, introduce in place of ϵ the auxiliary vector

$$\tau = \varpi^{-1}\psi\mu = \epsilon/\mathrm{S}\epsilon\psi\mu\,,$$

so that $$\psi\mu = n\phi\tau + \mathrm{S}\mu\psi\mu\ \psi\tau\,. \qquad (c)$$

(*a*) may now be written

$$\mathrm{S}\mu\phi\tau = 0 \qquad (a)$$

Hence (c) gives, by operating with $S.\mu$, $S.\tau$, and $S.\psi^{-1}\rho$,

$$S\mu\psi\tau = 1 \quad . \quad . \quad . \quad . \quad (1)$$

$$1 = nS\tau\phi\tau + S\mu\psi\mu \; S\tau\psi\tau \quad . \quad . \quad (2)$$

$$-1 = nS\rho\psi^{-1}\phi\tau + S\tau\rho \; S\mu\psi\mu \quad . \quad . \quad (3)$$

These preliminaries being settled, we must find the envelope of (b) subject to the sole condition (a). We have at once by differentiation

$$S\rho d\mu = 0 \text{, and } Sd\mu(\phi\tau - \psi\mu S\tau\phi\tau) = 0 \text{, so that}$$

$$x\rho = \phi\tau - \psi\mu S\tau\phi\tau , \quad . \quad . \quad . \quad (d)$$

Treat this with the three operators used before, and we have respectively

$$x = S\mu\psi\mu \; S\tau\phi\tau \quad . \quad . \quad . \quad . \quad (4)$$

$$S\tau\rho = 0 \quad . \quad . \quad . \quad . \quad (5)$$

$$xS\rho\psi^{-1}\rho = S\rho\psi^{-1}\phi\tau + S\tau\phi\tau \quad . \quad . \quad . \quad (6)$$

By means of (5), (3) becomes

$$-1 = nS\rho\psi^{-1}\phi\tau$$

so that (6) takes the form

$$xS\rho\psi^{-1}\rho = -\frac{1}{n} + S\tau\phi\tau \, . \; . \quad . \quad . \quad . \quad (6)$$

Substitute for $\psi\mu$ in (d) its value in terms of τ from (c); and x becomes, by (4) and (6), a factor of each term; so that

$$\rho = -nS\rho\psi^{-1}\rho \, . \, \phi\tau - \psi\tau \quad . \quad . \quad . \quad (d)$$

Eliminating τ between this and (5) we have finally

$$S \, . \, \rho(\psi + nS\rho\psi^{-1}\rho \, . \, \phi)^{-1}\rho = 0 \, .$$

(Equation (2), above, has not been, so far, required:—but it is necessary if we desire to find the values of $S\mu\psi\mu$ and other connected quantities.)

It is obvious that, if we had originally eliminated ϵ instead of η, we should have obtained the (apparently) different form

$$S \, . \, \rho(\phi + mS\rho\phi^{-1}\rho \, . \, \psi)^{-1}\rho = 0 \, .$$

It is an interesting example in the treatment of linear and vector functions to transform one of these directly into the other. (Tait's *Quaternions*, § 183.)

The Physiological Action of Eucain. By M. Charteris, M.D., Professor of Materia Medica, University of Glasgow, and William MacLennan, M.B., C.M.

Eucain, like cocaine, is a methyl ester of a benzoylated-oxypiperidine, carbo-oxylic acid. It is, however, a synthetic product, while cocaine is obtained exclusively from coca leaves; only, in the exhaustion, synthesis has been introduced for the purpose of completely exhausting the cocaine in the leaves.

The formula of cocaine and of eucain is the same.

Eucain, like cocaine, is insoluble in water, or nearly so, but the neutral salts formed by combinations with acids are soluble.

The hydrochloric salt of eucain appears in two forms.

One modification crystallises from water in small shining plates, which contain a molecule of water of crystallisation, and the formula for it is

$$C_{19}H_{27}NO_4HCLH_2O.$$

It dissolves in water to the extent of 6 per cent.

The *second* modification crystallises from a solution in methyl alcohol in shining prisms, which contain two molecules of methyl alcohol in crystallised form.

For anæsthetic purposes the first modification crystallised from water is to be preferred. A solution of it in water differs from a solution of cocaine in water in this respect, that it remains unchanged, while the cocaine solution, after a time, undergoes decomposition.

The appellation 'eucain' has no meaning. It is used simply to dispense with the complex chemical nomenclature which has been mentioned.

Eucain hydrochlorate is not so expensive as cocaine hydrochlorate.

Physiological Experiments.

Assisted by Dr MacLennan, I commenced in May last a series of experiments on the physiological action of solutions of the

hydrochlorate of eucain, and solutions of the hydrochlorate of cocaine.

Solutions of these salts were injected hypodermically into guinea-pigs of the same weight, and the results were compared.

At first the quantity used was small, but this was gradually increased, until the lethal dose of each was accurately ascertained.

After repeated experiments, we came to the conclusion that the lethal dose of eucain per kilo body-weight is ·09 gramme; the lethal dose of cocaine per kilo body-weight is ·068 gramme.

We also found that the mode of death by the two substances varied. With the cocaine salt we observed more rotatory movements of the head, more opisthotonos, more salivation and more laboured breathing, than with the eucain salt.

It was also noticed that the physiological action produced by a given dose of the eucain salt did not follow nearly so rapidly as that produced by a similar dose of the cocaine salt, under identical conditions with regard to the weight of the animal experimented on.

Hence the action of eucain is slower in onset, and less in intensity.

As illustrating this, we may state that ·0216 gramme of eucain hydrochlorate dissolved in 4 c.c. of water, when injected into a pig weighing 570 grammes, caused only a slight weakness of the hind limbs; the head was not affected.

After the expiry of a week an injection of the same strength of cocaine hydrochlorate was made into the same pig. In the course of half an hour there were seen, in the following sequence, clenching of the teeth, rotatory movements of the head, much salivation, quick and shallow respirations. Shortly afterwards, convulsions and opisthotonos occurred, which continued for an hour, after which time recovery was gradual. Recovery was, however, complete at the end of three hours.

After the expiry of another week the same pig had an injection of ·032 gramme of hydrochlorate of eucain in 4 c.c. of water.

In half an hour convulsions affecting the body, but with little disturbance to the head, commenced, and continued for 50 minutes. They were in no way so violent as those occasioned by ·0216 gramme of cocaine hydrochlorate; and after the convulsions ceased, recovery was perfect in the course of one hour.

Local Anæsthesia.

Three drops of a solution of hydrochlorate of eucain (1 in 10), when inserted into the eye of a guinea-pig, induced in 60 seconds complete anæsthesia of the cornea. The pupil was not affected, and there was no subsequent irritation.

It is apart from the nature of this paper to enter upon the utility of this new local anæsthetic. The clinical observations upon this are as yet meagre. But when used in operations on the eye, the evidence is clear that it has no effect on the pupil. Cocaine, on the other hand, dilates the pupil. Dr Berger of Paris, in operating for cataract, employs first a drop of a 1 per cent. solution, and after 3 minutes a drop of a 2 per cent. solution. This procedure, he says, causes complete anæsthesia of the cornea.

In dental practice it is found that 5 drops of a solution (1 in 10) injected into the gum before extraction of a tooth are sufficient to render this operation painless.

114TH SESSION—1896-97.

(Meeting December 7, 1896.)

PROF. JOHN GRAY M'KENDRICK, Vice-President, in the Chair.

Chairman's Opening Address.

The Council has done me the honour of requesting me to open the 114th Session of the Society, and to offer some remarks that may be deemed appropriate to the occasion.

PRESENT STATE OF THE SOCIETY.

There has been an unusual number of candidates for admission during the past Session, 26 Fellows having been elected. Of these nine are Professors and Lecturers in various Universities and Colleges, and eight are Doctors or Bachelors of Medicine. The total number of Ordinary Fellows is 513. At the corresponding date last year the number of Ordinary Fellows was 505.*

In the first place, I may be allowed to refer to some of the incidents in the not uneventful history of the Society during the past year.

LORD KELVIN'S JUBILEE.

In June last, the Jubilee of the appointment of our President, Lord Kelvin, to the Chair of Natural Philosophy in the University of Glasgow, was celebrated in that city by Delegates from the Scientific Societies of all parts of the world. This was an occasion unique in the history not only of the University on which Lord Kelvin's career has shed so brilliant a lustre, but also of any University in the United Kingdom. It is true that a few other Professors in our Universities have held office for fifty years, and have thus reached their jubilee, but never was there such a gathering to do honour to one man. The felicitations then offered were an expression of admiration of the splendid work Lord Kelvin has accomplished in Mathematical and Physical Science, and of the rare ingenuity and skill he has brought to bear on the invention of electrical and other devices which have contributed

* For these details, and also for the notices of deceased Fellows, I am indebted to the kindness of the Librarian, Mr James Gordon.

in no small degree to the enrichment and social progress of the world. Since Lord Kelvin—then William Thomson—became a Fellow of this Society, fifty years ago, he has contributed to its *Proceedings* and *Transactions*, up to 1891, no fewer than seventy-two communications, and we reflect with pride on the fact that several of his most famous papers were read to this Society. Only three of these will I venture to mention on this occasion. The Memoirs on Thermo-dynamics, the chief of which appeared in the *Transactions* from 1849 to 1851,* founded on the experimental work of Joule, and of Joule and Thomson together, established that department of science on a new basis. He was the first to investigate methodically and to restate the whole subject from the mathematical point of view; he developed the principles of Carnot so as to give the views now current as to the nature of heat; and when he showed how to reckon temperature on an absolute thermo-dynamic scale, the dynamical theory of heat secured its position as a recognised branch of physical science. Three years later, in 1852,† Thomson announced the principle of the dissipation of energy, and showed that a part of the energy of the universe is being slowly but surely changed into uniformly diffused heat, and therefore ceases to be available for mechanical effect. From this principle stupendous consequences flow, and the consideration of these has had a marked influence on human thought. Lastly, the papers on Vortex Motion, the first of which was read in February 1867,‡ applied our knowledge to the properties of vortex rings, first investigated by Helmholtz, towards the conception of an atom. Imagining a fluid having only the properties of inertia, invariable density, and perfect mobility, the motions of the fluid are submitted to mathematical analysis, and on this is founded a theory which may not only give a new expression to the atomic theory, but may be applied even to gravitation. From these contributions have flowed many fruitful results, which no doubt will be recorded by the historian of science.

* W. Thomson, "On Carnot's Theory of the Motive Power of Heat," *Trans. Roy. Soc. Edin.*, 1849; "On the Dynamical Theory of Heat," *Trans. Roy. Soc. Edin.*, 1851.

† W. Thomson, "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy," *Proc. Roy. Soc. Edin.*, 1852.

‡ W. Thomson, "On Vortex Atoms," *Proc. Roy. Soc. Edin.*, 1867.

The jubilee was the climax, not the end, of a great career. In the remarks made by Lord Kelvin himself on that occasion, nothing was more striking than the tones of buoyant hope that rang through every sentence, and one felt that the speaker's intellectual eye was not dim nor his natural force abated. The Fellows of this Society join in the hope that Lord Kelvin may yet see many days, and that he may still be able to use his powerful intellect and rare inventive faculty in the service of science. To this I must add that not a few among us have come under the spell of his personal friendship, and to us he is even more than the mathematician or the philosopher.

Appointment of Delegates and Representatives.

In compliance with an invitation, the Council appointed two of its members, Dr Hunter Stewart and Mr Mossman, to represent them at the meeting of the Sanitary Association held at Newcastle.

The Council also appointed one of its Secretaries, Dr John Murray, to represent them at the International Fishery Congress that met last September at Sables d'Olonne, in France, a request having been made that the Society should send a Delegate.

Dr John Murray and Dr Noel Paton were appointed to represent the Society at the opening of the Gatty Marine Laboratory at St Andrews on the 30th of October last. We all wish much success to a Laboratory so advantageously placed for the study of the Fauna of the Firth of Forth.

You will be aware that a member of Council, Professor D'Arcy Thompson, was appointed by the Government a Commissioner to report on the aspects of the Seal Fishery in Behring's Straits, and on its international relations.

Papers of last Session.

I shall not, on this occasion, attempt to give an analysis of the many important communications made to the Society. Suffice it to say that they dealt with many subjects,—Mathematical, Physical, Physiological, Zoological, and Meteorological, and also relating to Public Health.

Dr Anderson's Discovery.

It is interesting to mention that the Rev. Dr Anderson, who

made the notable discovery of the star Nova Auriga, and who regularly consults the astronomical works of the Library, has in the course of last session discovered three variable stars, not previously known to vary. These are:

Variable Stars.

The first is in Lyra. +36° 3066 of Bonn Durchmusterung. R.A. and Decl. for 1855·0, 18 h. 9·9 m., and +36° 37′. Variability confirmed by Mr Paul S. Yendell, of Dorchester, U.S.A., the well-known authority on variable stars.

The second is in Hercules. +27° 2772 of Bonn Durchmusterung. R.A. and Decl. for 1855·0, 17 h. 5·0 m., and +27° 14′.

The third is in Hercules. Not in Bonn Durchmusterung. R.A. and Decl. for 1855·0, 16 h. 4·1 m., and +25° 28′.

Obituary Notices of Ordinary Fellows.

During the past Session thirteen of our Ordinary Fellows and five of our Honorary Fellows have died. It is usual on the opening of a new Session to advert to each of the deceased, it being understood that the brief records I shall now give are in nowise intended to supersede more elaborate obituary notices, which I hope we will receive.

The first to be taken away was Hugh Miller, the younger and only surviving son of the famous geologist of the same name. Born in 1850, he was educated at the School of Mines, to which he was nominated by Sir Roderick Murchison on account of his father's important contributions to geological science. He joined the Geological Survey of England and Wales about the year 1874, and one result of his researches was a paper on Tynedale escarpments. He was afterwards transferred to the Survey of Scotland, and was chiefly employed mapping out the old red sandstone of the Moray Firth basin. His work, as was seemingly appropriate, was confined chiefly to Cromarty and Sutherlandshire. From his father he inherited literary ability, and he was always painstaking and accurate in scientific work. He died on 9th January 1896.

Josiah Livingstone was born in Edinburgh. Notwithstanding the responsibilities of an important mercantile business, he took a

prominent part in the management of numerous public institutions. In 1881 he was elected Master of the Merchant Company, in succession to Sir James Falshaw. The Merchant Maiden Hospital being superiors of the town of Peterhead, he took a leading part in the movement, which came to a successful issue, for having a national harbour of refuge erected at Peterhead by convict labour. He presided over the Edinburgh Chamber of Commerce from 1869 till 1872, and he was again elected to that position in 1879. As Chairman, he was present as the Representative of the Chamber at the opening of the Suez Canal in 1869. He was Chairman of the Edinburgh Literary Institute from 1873 till his death. He died on 13th January 1896.

George Robertson was the eldest son of the late Hon. Hercules James Robertson, Lord Benholme, one of the Senators of the College of Justice. As an engineer, Mr Robertson carried out the construction of Leith Docks, and was well known in connection with marine and river engineering. He was admitted a Fellow of this Society in 1859, and was a member of the Institution of Civil Engineers. He died on 7th February 1896, at the age of 65 years.

Robert Lawson was born at Kirriemuir. Connected originally with journalism, he left that profession, and studied medicine at the University of Edinburgh, where he took his degree of M.B. in 1871 and of M.D. in 1888. He was successively Assistant to the Chair of Practice of Medicine, Assistant Medical Officer of the West Riding Asylum, Yorkshire, Medical Superintendent of Wonford House, a registered hospital for the insane in Devonshire, and in 1878 he was appointed a Deputy Commissioner in Lunacy for Scotland. He was a large contributor to medical journals, but latterly his writing was confined to his Annual Reports to the General Board of Lunacy on the Care of the Insane in Private Dwellings. He was a man of refined literary tastes, an extensive reader in many departments of literature and science, and of a kindly and generous nature. He died on 22nd February 1896.

David Cunningham was born in Dundee on the 14th July 1838. He was educated first at the High School of Dundee and afterwards at the Queen Street Institution, Edinburgh, after which he served an apprenticeship with Messrs Blyth, Engineers, Edin-

burgh. Subsequently he was appointed Resident Engineer on the Portpatrick Railway. When this was completed, he was appointed Resident Engineer on the Galashiels and Peebles line. In 1869 he was appointed Engineer to the Dundee Harbour Trust, which post he held for the remainder of his life. Among other works which he executed while thus engaged in his native town, he deepened the bed of the Tay 10 feet opposite the Harbour Works, constructed a large graving dock, and invented a large caisson for use at dock entrances. He died on 13th June 1896.

The Rev. Thomas Milville Raven studied at St David's College, Lampeter, and received the Lambeth degree of M.A., conferred by the Archbishop of Canterbury. He was ordained in 1852. After serving in various curacies for sixteen years, he was, in 1867, appointed Vicar of St Gregory's, Crakehall, with St Mary Magdalene's, Langthorne. He was made Surrogate of the Diocese of Ripon in 1878. Some years ago he built, entirely at his own expense—about £3000—St Mary Magdalene's Church at Langthorne. He maintained this church at his own cost besides meeting part of the expenditure in connection with St Michael's Church, Crakehall. His favourite occupation was photography, in which he acquired proficiency, and one year he won the first prize medal of the British Photographic Society. He was a member of the Yorkshire Archæological Society. He died at the age of 68.

James Abernethy, once President of the Institution of Civil Engineers, was born at Aberdeen on 14th June 1814, and was at first educated in a school in that city, and subsequently, along with his brother George, at Cotherstone, near Barnard Castle, which is believed to be the original of Charles Dickens' description of Dotheboys Hall. Whether Cotherstone was the identical school or not, the system was at any rate similar. Fortunately, one day a clerical uncle visited his nephews, and realising their unhappy condition, took them to his manse near Haddington, where they attended the Grammar School. In 1833 James Abernethy went to Sweden to investigate a manganese mine, and remained there for four years. He was afterwards employed as Assistant Engineer at the Goole Docks, the Aire and Calder Canal, and the railway

between Wakefield and Leeds. In 1840, he was appointed Resident Engineer at Aberdeen Harbour. As Surveying Officer under the Preliminary Inquiries Act, he reported, between 1844 and 1852, on the condition of the Clyde, the Tyne, the Ribble, and the Ports of Glasgow, Liverpool, and other places. At Birkenhead his scheme of a floating dock was adopted, after the failure of Rendel and Robert Stephenson's scheme of subaqueous sluices. In 1854, he took an office at Westminster, and was much engaged as a Consulting Engineer. It is impossible in this brief sketch to enumerate the various works, principally of marine engineering, which he accomplished in England, Ireland, and in Egypt. He was elected President of the Institution of Civil Engineers on 21st December 1880. He died on 8th March 1896.

THOMAS DAWSON BRODIE, the eldest son of John Clerk Brodie of Idvies, C.B., LL.D., was born on 26th December 1832. He attended Edinburgh Academy and afterwards Harrow School, completing his education at the University of Edinburgh. He was admitted a member of the Society of Writers to the Signet on 12th November 1857, and afterwards became senior partner of the firm of Gibson Craig, Dalziel & Brodies, and Secretary to the Carron Iron Company. He acted as Deputy-Keeper of the Privy Seal from 1869 to 1874. On the death of his father, about nine years ago, he succeeded to the estate of Idvies, and a Baronetcy was conferred on him in 1892. He was a lover of art, and was possessor of a fine collection of ornithological specimens. He died on 6th September 1896.

The Rev. JOHN GIBSON CAZENOVE, D.D., came of a family that claimed to be a cadet branch of the De Cazenoves of Guienne. He was educated at the Royal Free Grammar School, Marlborough, and at Brasenose College, Oxford, where, in 1841, he obtained a prize for the best Latin and English essays. In his earlier career he endeavoured to train himself for the Bar, but was induced to take holy orders. He was Provost of the Theological College, Cumbrae, from 1867 to 1875. Dr Cazenove came to Edinburgh in 1878 as Sub-Dean and Chancellor of St Mary's Cathedral. For the *Encyclopædia Britannica* he prepared the article on "Mohammedanism," and among his other published works is *Historic Aspects of the a priori Argument concerning the Being and Attri-*

butes of God. He was a man of subtile insight and wide learning. He died on 30th September 1896.

John Pender was born in 1816 in the Vale of Leven, Dumbartonshire, and studied the classics and modern languages, turning his attention also to science and art. He distinguished himself in the drawing classes, and was awarded a gold medal for an original design. In his twenty-first year he became general manager of a factory near Glasgow. In Glasgow, and afterwards in Manchester, he established a business as export merchant. His foreign connections as a Manchester merchant were world-wide, including, in particular, the United States, South America, China, and India. He was one of the few who perceived the practicability of connecting this country with America by submarine telegraphy. Three hundred and forty-five gentlemen resolved to contribute £1000 each for this purpose. He became Director of the Atlantic Company. Two of their cables were lost within a few hundred yards of the Irish coast. About the end of 1858 one cable was actually laid, but it refused to work after the trial messages. The Great Eastern was next sent out, with a cable constructed at a very high cost, but the ship parted with her cable in mid-ocean, and could not that year endeavour to recover it. The Gutta Percha Companies that were asked to supply a new cable required a guarantee of a quarter of a million, and Mr Pender offered his personal guarantee for that amount, which was accepted. The effort now made was completely successful. A working cable was laid in 1866; the lost one was recovered. He thereafter became chairman of the companies which laid the first cable to India and the Mediterranean lines from Gibraltar to Malta, and thence to Egypt and Turkey; and developed the cables to China, Australia, Africa, Brazil, and the Argentine Republic. The capital invested in this gigantic network of communication is estimated at over forty millions sterling. The eleven cables in the Atlantic alone cost over fourteen millions sterling.

In 1888, he received the honour of K.C.M.G., and, in 1892, the honour of G.C.M.G. He represented Totnes in the House of Commons from 1862 to 1866. He afterwards represented the Wick Burghs from 1872 to 1885. In 1892, Sir John was again elected for those Burghs. It was mainly through his instrumentality

that the town of Wick was relieved of a debt of about £150,000 to the Government in respect of a breakwater, which debt had crippled the energies of the town. He was elected a Fellow of this Society in 1869, and died on 7th July 1896. His characteristics were untiring industry, clear foresight, a capacity for dealing with finance, determined perseverance, and good common-sense.

James Clerk Rattray was born on 21st January 1834. He was educated at the High School of Edinburgh, and by private tutors. He took his degree at Edinburgh University. He went into the Army Medical Service, and served for some years at Gibraltar and Malta. He retired in 1864. He never was in private practice. He was elected a Fellow of this Society in 1885, and died on 22nd February 1896.

George M'Roberts was born in 1839, and was chiefly educated in the Falkirk Grammar School. For many years, in spare hours, and amid discouragements, he studied chemistry, more especially in its technical aspects, and in 1870 he established a chemical manufactory at West Quarter, near Redding. About 1873, he became associated with Mr Alfred Nobel, and took charge of the factory established by Messrs A. Nobel & Co. at Ardeer, near Irvine, in Ayrshire. This position he held for about fifteen years, when ill-health obliged him to resign. During these years he laboured unceasingly in developing the technical methods for the manufacture of explosives. The first pound of dynamite, on a manufacturing scale, was made by Nobel and M'Roberts, and to the latter are chiefly due the admirable arrangements at Ardeer by which this dangerous substance can be made in large quantities with a maximum of safety. Dynamite was succeeded and partly supplanted by blasting gelatine, a superior explosive, and M'Roberts brought his experience and inventive talents to bear on the making of this substance. When illness came—an insidious form of paralysis—an illness probably in no small measure due to his arduous life, his mental faculties were untouched, and he was able to take a leading part in the planning of the Government Factory at Waltham Abbey, at which the still more modern explosive Cordite is now made. A busy life, spent in the superintendence of technical details, did not leave much time for publishing papers, but there is one on "*The Manufacture of Blasting Gelatine*" that

merits notice. During the years of retirement, while life was slowly ebbing away, he spent his days cheerfully, showing to the last many of those qualities of head and of heart that gained for him success in life and the affection of many friends. He died on 15th January 1896.

Obituary Notices of Foreign Honorary Fellows.

The following five Foreign Honorary Members of our Society have died during last Session :—

Gabriel Auguste Daubrée was born at Metz on 25th June 1814. From the École Polytechnique he passed into the Corps des Mines. He devoted himself chiefly to the experimental side of Geology, and expressed his deep obligations to our former President, Sir James Hall, the founder of that branch of the science, whose two papers on the subject, published in our *Transactions*, he highly appreciated. Daubrée devoted himself to solving the difficult problems of metamorphism by actual experiment. He also took especial interest in meteorites, and carried on a series of experiments in order to reproduce their characters artificially. He also published some volumes on the phenomena of underground water, and traced the various changes which water is effecting within the crust of the earth. He held official posts in the École des Mines and the Muséum d'Histoire Naturelle. As a member of the Académie des Sciences he accompanied the excursionists from that body to Chantilly on the occasion of the centenary of the Academy, and was welcomed by the Duc d'Aumale as an old colleague and personal friend. He presented a copy of his great work *Études Synthétiques de Géologie Expérimentale* to the Library of this Society. He died at Paris, at the age of 82 years, on 29th May 1896.

August Kekulé was intended by his father to be an architect, and for that purpose underwent a preliminary training at Giessen. At that town, however, he came under the fascinating spell of Liebig, and renounced architecture for Chemistry. He afterwards went to Paris, and there studied under Regnault, Frémy, Wurtz, and Gerhardt. After engagements at Reichenau and at St Bartholomew's Hospital, London, he returned to Germany, and started a small laboratory at Heidelberg, where he carried out his work on

the fulminates and on cacodyl. His first call as Professor was to Ghent; from that University he was translated to Bonn. It was Kekulé who first gave definite form to Frankland's conception of valency, and he applied this idea to carbon compounds. Out of the conception of valency, too, grew his theory of cyclic compounds, said to have been prolific to a degree almost unparalleled in the history of pure science. He died on 13th July 1896.

ERNST CURTIUS was born at Lubeck in 1814, and studied at the College of his native town, and at the Universities of Bonn, Göttingen, and Berlin. In 1837, he visited Athens in company with Professor Brandes, in order to begin his famous researches into Greek antiquities. He took a leading part in archæological expeditions to the Peloponnesus and Olympia. He taught for some time in the Colleges of Berlin, and became tutor to the Emperor Frederick. In 1856, he succeeded Hermann as Professor of Greek at Göttingen. Since 1870, he had been Director of the Royal Museum there. His best known works relate to Greek antiquities. His *History of Greece* has been translated into English. He was a Foreign Associate of the French Academy of Belles Lettres and Inscriptions, and was elected an Honorary Fellow of our Society in 1889. He died on 11th July 1896.

HUBERT ANSON NEWTON was born in 1830, and died 12th August 1896. His reputation is largely connected with the history of the November showers of meteors. His collection and discussion of the original accounts of thirteen meteoric displays, distributed over a period of more than nine hundred years, demonstrated the permanent character of the phenomenon. Perhaps one is not justified in calling him the Director of the Yale Observatory, as he only held that position for two years, 1882–84; and he seems to have preferred the position of Secretary to the Board of Managers. The work that issued from the Observatory under his management, whether it be parallactic inquiry or stellar triangulation, placed the institution in the front rank of those devoted to extra-meridional work. I will only further allude to his inquiry into the capture of comets by Jupiter or other planets, in which he has shown that the perturbing action of the planets on parabolic orbits tends to produce elliptic orbits of short periods, moderately inclined to the ecliptic. He was elected an Honorary Fellow of this Society in

1886, and an Honorary Fellow of the Royal Society of London in 1892.

Armand Hippolyte Louis Fizeau was born in the year 1819. His experimental method of determining the velocity of light by a rotating wheel is known to every student of optics. Another experiment with which his name is honourably associated is that by which he determined the amount of drift of light-waves in a transparent medium in motion. He made some remarkable experiments on the number of interference bands observable with approximately homogeneous light, and on light in different parts of the field of illumination in interference experiments. He was elected a Foreign Member of the Royal Society of London in 1875, and of our Society in 1892. He died September 18th, at the age of 77.

Obituary Notices of British Honorary Fellows.

The following two British Honorary Fellows died during last Session:—

John Russell Hind was originally intended for the profession of engineering, for which he had little taste; and it was fortunate that circumstances permitted him to join the staff of the Royal Observatory, where he was attached to the magnetical and meteorological department. In 1844, he left Greenwich to take charge of Bishop's observatory at Regent's Park. At that time Neptune was not discovered, and he began, at that observatory, to form ecliptical charts of stars, with the view of detecting the object that disturbed the motion of Uranus. The comparison of these charts with the heavens led to the discovery of a number of small planets, some variable stars, and a few comets. His facility as a computer led to his selection for the post of Superintendent of the Nautical Almanac when a vacancy occurred in 1853. He received the medals of the Royal and Astronomical Societies. He was a Corresponding Member of the Institute of France, a Fellow of the Royal Society of London, and was elected a Fellow of this Society in 1895. He died December 23rd, 1895.

William Robert Grove was born at Swansea in 1811. From the age of 25 to 50, Grove, though practising the profession of the law, was actively engaged in scientific work. Prior to the publication of his great work *On the Correlation of the Physical Forces*

which appeared when he was 35 years of age, he had devised his voltaic cell known as the "Grove," and other voltaic combinations. The "Grove" was in its day an important invention, as giving high electromotive force and moderate resistance. The polarisation of gases occupied much of his attention, and he invented the gas-cell, interesting as the forerunner of the modern secondary battery. He received a medal from the Royal Society for his Paper on *Voltaic Ignition and the Decomposition of Water*, which formed the Bakerian Lecture for 1846. In 1871 he became a Judge, and shortly afterwards received the dignity of knighthood. He was D.C.L. of Oxford and LL.D. of Cambridge. He was elected a Fellow of the Royal Society of London in 1840, and a Fellow of this Society in 1881. He died August 2nd, 1896.

When the Council requested me to deliver this opening address, they suggested that I might occupy a portion of it with an account of any recent investigations that have occupied my attention. It gives me pleasure to act upon this suggestion, and I therefore venture to bring under your notice a few subjects that have specially interested me during the last few months.

The Structural and Physiological Nervous Unit.

Up to a comparatively recent date, our conception of the nervous system was that it was built up of nerve cells and nerve fibres, more or less intimately bound together by a peculiar kind of tissue known as neuroglia. It was further supposed that, in the central nervous organs, nerve cells were linked together by processes passing from one cell to another, that sensory nerve fibres passed into, and were in their substance continuous with, nerve cells, and that motor fibres originated in nerve cells, and passed out to muscle fibres. It was also held that the elements of sensory organs, such as the retina or the organ of Corti, were organically connected with nerve cells in the cerebral organs. In short, the nervous system, as a whole, was held to be composed of cells and fibres closely connected together, so that the structure was like a vast web, the size of the meshes of which would vary according to the intricacy of the connections by which the various cellular elements were held together by nerve fibres. These histological

conceptions were founded on the microscopical scrutiny of sections prepared by the older methods of hardening and staining, from the time of Lockhart Clarke to nearly the present day.

The notions of physiologists, as is usually the case, were more or less in conformity with, and were influenced by, these histological conceptions. Nerve cells were supposed to be excited by nervous impulses, or to originate nervous impulses, and nervous impulses appeared to pass from cell to cell. Many illustrations, some fanciful, were culled from the nomenclature of electrical science. Notions also of more or less "resistance" in the passage of nervous impulses passed into current use. These notions were founded on the statement of the histologists that there was continuity of nervous elements, and they were derived from a fanciful analogy to the passage of electricity along conductors. No one offered proof of anything similar to resistance (in the electrical sense) occurring in a nerve; and the term resistance, in nervous phenomena, was employed to explain certain cases of delay in the transmission of a nervous impulse in centres, and of an apparent choice of path by the nervous impulse. Further, there was no recognition of a nervous structural or physiological unit, unless in the separation of the nervous elements into cells and fibres; and these two were often discussed as if they were separate and distinct.

In science, the invention of a new method of research, by bringing to light new facts, or by upsetting notions founded on incorrect observation, leads not only to a critical examination of older ideas, but the formation of new ideas. A new method marks a new step in progress. For a time, things seem to be in confusion; old pathways have to be abandoned and new ones opened up; and it is not until a fresh theory has been devised to meet and embrace the new facts that men feel they are again on safe ground.

With regard to the physiology of the nervous system, we are at present passing through such a critical, and, it must be confessed, a dark period, in consequence of an entirely new set of facts having been elicited by a new method, now well known to histologists, in the preparation of sections of the nervous organs for microscopical examination. The method, first devised by Golgi, and applied with much success by Ramon y Cajal, Retzius, and many other observers, consists in acting on the nervous organs with bichromate

of potash, osmic acid, and nitrate of silver. The interaction of bichromate of potash with nitrate of silver results in the formation of chromate of silver, which is precipitated in the form of a dense blackish matter on the surfaces of nerve cells and on the processes connected with these. In this way numerous fine fibrils are shown, most of which escaped detection by the older methods, and a large number of new and important facts have thus been brought to light. These facts are now described in almost every text-book, but their significance and importance have not yet been appreciated even by physiologists, and still less by those who are interested generally in physiological science, more especially in its bearings on psychological questions. This must be my excuse for bringing a matter forward in this address which has lately occupied much of my attention.

A nerve cell, with its prolongations or processes, may now be regarded as a nervous unit, to which Waldeyer has given the name of *neuron*. The cell is a mass of protoplasm, showing usually in its interior numerous fine fibrils, which enter it by each process, and cross and re-cross in all directions, so as to form a fine and irregularly-meshed network. From the cell arise numerous processes or poles, to use the old term. With the exception of the bipolar cells in the ganglia on the posterior roots of the spinal nerves, and a few others, all nerve cells show numerous processes, or they are multipolar. These processes also appear to be fibrillar in their texture, and they are of two kinds:—(1) Protoplasmic processes, or *dendrites*, which, by dividing and subdividing, form a very fine set of twigs, like the twigs on the branchlets of a tree as seen against a winter sky. This dendritic appearance is called an *arborization*. (2) A thicker process, of variable length, which is now known to be the *axis-cylinder* of a nerve fibre. In the cerebral nervous organs these axis-cylinder processes may be very short, amounting to only one or two millimetres, but axis-cylinders may extend from cells in the anterior cornua of grey matter in the cord to muscles in the limbs, and they may then reach a length of many millimetres. The axis-cylinder process also terminates in a smaller kind of arborisation, by which it comes into relation with (1) a motor apparatus, such as the contractile fibres of a muscle; (2) a secretory apparatus, such as the cells in the acini of a

gland; or (3) with the dendrons of another neuron in the immediate vicinity of the first, or at a great distance from it. It is to be noted, however, that the arborization of two axis-cylinders never come into close relationship with each other. Further, no direct *continuity* can be established between neurons. The dendritic processes may closely approach the terminal arborizations of an axis-cylinder; and, in turn, the terminal arborizations of an axis-cylinder may closely approach the dendritic processes of another neuron, or the body of a nerve cell, or the cells of a secreting gland, or, as in the end plates in muscle, the contractile tissue, but there is never anatomical continuity. If we could detach a neuron, and pick it out from its bed nothing would be ruptured. Another remarkable fact brought out by the new method is that both axis-cylinders and dendritic fibrils often give off at right angles extremely fine fibrils, and that these fibrils end in delicate dendritic-looking processes by which they are brought into relation with other neurons.

Further, the researches of Golgi, Cajal, Kölliker, and many others, appear to show that all the central nervous organs are built up of neurons, each constructed on the same type, and that the varying degree of complexity of the structure of any part of the cerebral nervous organs depends on the complexity in detail of the neurons composing it. Thus, spinal cord, cerebellum, cerebrum, and the nervous portion of the special terminal organs of sense, such as the retina, the organ of Corti with the ganglion of the *lamina spiralis*, are all constructed on the same general plan. They are aggregations of neurons, arranged with reference to each other in a special way. It also appears that one portion of a neuron may be a considerable distance from another. Thus a neuron in one of the grey layers of the cerebrum may be related, by its dendritic processes, to the upper layers of the cortex, and, by its axis-cylinder process, and the terminal arborizations of the latter, to the dendritic processes of a motor neuron in the anterior horn of grey matter in the lumbar region of the spinal cord. It appears to me that a great step has been taken in this recognition of a morphological nervous unit.

We now turn to the physiological side of the question. Writers are, on the whole, of opinion that nervous impulses are conveyed

by the dendritic processes always *towards* the cell, and by the axis-cylinder and its terminal arborizations always *from* the cell. In my opinion, the first statement at present rests on insufficient proof, and it will be noticed that, if we admit the second, it upsets the older notion of a sensory impulse travelling along a sensory nerve from the skin ultimately reaching a nerve cell, say in the cord or brain. Further, this statement carries with it a number of startling conclusions. For example, if the dendritic processes always carry impulses towards the cell, the numerous fine axis-cylinders in an area of skin, which supply sensibility to that part, must be regarded as the dendritic processes of a bipolar cell in a ganglion on the posterior root of a spinal nerve, and the sensory filament we have hitherto regarded as an axis-cylinder as a specially enlarged division of the dendritic arrangement. If this be so, then the axis-cylinder beyond the ganglion is the true axis-cylinder, and its arborization in the cord, by which it may become related to another neuron, is the homologue of the end plates at the termination of a motor axis-cylinder. Again, if we trace sensory fibres up the cord and onwards to the cerebral cortex, these fibres are true axis-cylinders, and they end in fine fibrils (again homologous with the end plates in muscle), by which an arborization is formed, bringing them into relation with the dendritic processes of a neuron, the body of which is in the grey matter of the cortex, and which is presumably motor.

There is also a difficulty in understanding the true significance of the fine fibrils, already alluded to, which pass off at right angles from the main stems. If these come off from fibrils that are continuations of the dendron, as they do in the grey layers of the cerebrum, do they conduct nervous impulses downwards and outwards to the dendrons of adjacent cells, or do they carry impulses from such cells to the cell from the dendron of which they spring? and if they come off from the true axis-cylinder process, do they conduct outwards, so as to bring their parent cell into connection, by terminal arborizations, with other structures?

Again, if there is no continuity of structure, and only *contiguity of terminations*, how is a nervous impulse transmitted from one neuron to another? Some have suggested that this may occur by actual movements of the terminations, in a manner analogous to

those of the long pseudopodia found in many of the lower invertebrates. No one has seen such movement, and the only movement that has been followed is that of growth. Cajal is of opinion that both the dendron processes and the terminal arborization processes actually grow outwards, like the rootlets of a tree, in correspondence with the advancing development of the individual in fœtal and early life. A more likely supposition is that neuron may act on neuron by an electrical excitatory process. It is well known that such processes occur in contractile muscular fibres, and that one muscular fibre set in action by a nervous impulse reaching its end plate may generate excitatory electrical changes which stimulate adjacent fibres that do not possess end plates. Such a process may exist in neurons, but it has not yet been demonstrated.

The most remarkable example of the branching arrangement of a neuron with which I am acquainted occurs in *Malapterurus electricus*, the Räāsh or the Thunderer-fish of the Arabs, a live specimen of which was shown to this Society by Professor Goodsir in 1855.

In this animal the electric organ forms a layer beneath the skin, enveloping the body with the exception of the head and fins. Its structure, as worked out chiefly by the laborious researches of Fritsch of Berlin, is that of a honeycomb-like tissue, in which, however, the spaces are not hexagonal, but lozenge-shaped. Each lozenge has, of course, four sides, and on two of these, adjacent to each other, we find an electric tissue consisting of granular protoplasm, and not unlike that of the electric disks in other electrical fishes. Fritsch regards this electric tissue as a layer of modified epithelial cells. The electric organ, therefore, is an altered condition of the skin glands. In the epidermis of the thunderer there are peculiar club-shaped cells, which are forms transitional into true electric cells, and, towards the tail, lozenge-shaped spaces occur in which there is no electric tissue. The number of these electric cells is enormous. After counting the number in one cubic centimetre of the organ (a cube, each side of which measures two-fifths of an inch), Fritsch computes the total number in the organ of a full-sized fish, and brings up the total to two millions. Each electric cell is supplied with an individual nerve filament which enters the lozenge at one angle and loses itself in the electric

tissue. These filaments are formed by the division of larger ones, and, strange to tell, when the filaments of one side of the body are traced inwards to the spinal cord, they are all found to spring from one single nerve filament which originates in a single gigantic nerve cell or neuron. This nerve cell has numerous protoplasmic processes which coalesce here and there to form a perforated plate, in the meshes of which capillaries may be seen. These giant electric cells, of which there are two lying side by side, one in each lateral half of the cord, and connected by a filament passing across the cord, are about the $\frac{1}{125}$th of an inch in diameter. We have thus the whole electric organ supplied by two neurons, a fact unique in science. Further, the electric cells of one half of the body are each supplied by a nerve filament, that is to say, there are, in the periphery, 1,000,000 individual filaments. These, however, all spring from *one* nerve filament, starting from the single neuron. Fritsch, with praiseworthy zeal, measured the diameter of the parent nerve filament, and compared it with the sum of the diameters of the million of filaments in the periphery. He found that the ratio was as 1 is to 364,000, showing the remarkable fact of a gradual increase in the amount of matter forming the axis-cylinder as we pass from the spinal nerve cell to the ultimate nerve fibril. The single electrical cell, little more than the $\frac{1}{120}$th of an inch in diameter, can thus discharge the whole battery on one side of the body.

Recently Professor Gotch has had the opportunity of making observations with the capillary electrometer on the electric discharges of this fish, and he has photographed the effects following *one* single stimulus applied to the axis-cylinder. The effect is not *one* electrical discharge, as might have been expected, but eight or ten, in a series gradually diminishing in intensity. Here we have a kind of electrical reverberation; and, by a careful study of the time relations of these discharges, Gotch is of opinion that the effects are chiefly due to changes occurring in the numerous nerve fibrils branching off to the electric cells. Suppose we could start a Gatling gun by one movement, then the barrels would go off, one after the other, as do the electric discharges given off by the Räāsh.

These new details of structure also give rise to many difficult questions as to the individuality of neurons. Is each neuron a

separate individual, a cell with pseudopodic-like processes peculiarly differentiated? Has it a life of its own; if so, has it a consciousness of its own; and is our consciousness related to the conscious states of the millions of neurons in the brain? It must be confessed that these new investigations have not made it easier but more difficult to comprehend the mode of action of nerve centres, and as I examine microscopical specimens showing the neurons of the cord, or the still more complicated arrangement of neurons in the cerebrum, and the baffling complexity of the cerebellum, I feel bewildered in attempting to understand how these structures act. What we now need is a satisfactory physiology of an individual morphological nervous unit, a neuron, and I shall be glad if these remarks stimulate younger workers to investigate this most difficult subject.

Experiments on the Rhythmic Stimulation of Sensory Nerves of the Skin.

Suppose we introduce a telephone *a* into the primary circuit of an ordinary induction coil, such as used in physiological laboratories, and another telephone *b* into the circuit of the secondary coil, and suppose the wires connecting telephone *a* with the primary and connecting telephone *b* to be many yards in length. If one observer speaks into *a*, another observer will hear every word if *b* is applied to his ear. Every one is acquainted with this fact, that the currents awakened in *a* induce corresponding currents in the secondary coil, which are transmitted to *b*. If we substitute for *a* a microphone-transmitter *c*, and speak to it, the tones are reproduced by telephone *b*.

Further, as I showed to the Society in February last, if we suspend a microphone-transmitter over the phonograph, when the latter is in motion and giving out tones, the variations in resistance in the microphone-transmitter produce such alterations in the current flowing through it to the coils of an electro-magnet as to make it possible, mechanically, to record these variations.

These considerations led me some months ago to try the following experiment. The microphone-transmitter was suspended over the phonograph disk, and it was introduced into the circuit of the primary coil of the induction machine along with four Obach's

cells (Q type). The terminals of the secondary coil were carried to two strips of platinum foil immersed in two insulated vessels (glass beakers or shallow vulcanite troughs) containing a ·75 per cent. solution of common salt or sulphuric acid (1 : 10 of water). I then found that when the phonograph was set in action, and when the fingers were immersed in the vessels, an electric thrill was felt in the fingers which corresponded to the rhythm, time, and intensity of the tune played by the phonograph. By carefully graduating the strength of the induction shocks, each note and chord of the music can be distinctly felt.

The experiment at once suggested a number of very interesting questions. Each variation in resistance in the microphone-transmitter produced by the varying pressures caused by the waves of sound was followed by a corresponding variation in the strength of the stimulus from the secondary coil, and the sensory nerves of the skin were capable of appreciating all these variations. The effects are most striking when the strength of the stimulation is such as not to be sufficient to excite muscular twitchings. Only a "thrill" should be felt.

In the first place, what structures in the skin are stimulated by such electrical thrills? In the skin we have, richly distributed, numerous fine sensory nerve filaments. Whether these all terminate in end organs or not is a question that cannot at present be decisively answered, but the evidence rather favours the view that they do not all terminate in end organs. Sensations of touch and of temperature have been experienced and referred to small areas of skin which have been excised from his own body by the ardent experimentalist, and in which no end organs have been found by the aid of the microscope. To the skin we refer at least two well defined sensations, touch (which is essentially a sense of pressure) and temperature. Touch is believed to be connected with various kinds of touch corpuscles, but no end organ has yet been discovered for the sense of temperature. Further, in a given area of skin (say of the fingers) points may be found connected with touch, that is to say, tactile sensations are experienced when pressure is made on these points, while no sensation of touch follows pressure on a point a millimetre or two to one side of the tactile spot. In the same area there may also be detected what are

now termed "hot spots," and near these other spots called "cold spots." A copper point, of the temperature of the skin, applied to a cold spot, excites a sensation not of heat, but of cold, and, on the other hand, the same rod applied to a hot spot excites a sensation of heat. So we have pressure spots, hot spots, and cold spots. These spots are not indiscriminately mixed, but there are little adjoining areas in which one of the three kinds predominates. Temperature spots are insensitive to pain, that is to say, a fine needle may be pushed into such a spot without causing pain. Further, as was shown by Stanley Hall and Donaldson, in 1885,* when a metallic point conveying a current of electricity was carried along the surface of the skin there was considerable diversity of sensation at different points, so that the authors observed cutting-pain points, thrill points, tickle points, points at which the sense of motion of the irritating body appeared to be increased, and blind or dead points, to which no sensation was referred. This is the only observation I have met with bearing on the effects on sensations produced by weak electrical stimulation of the skin. It would appear, therefore, that a dermal sign (that is, the effect in consciousness of irritation in one way or another of an area of skin) is often a mixture of feeling, and that this mixture may give a characteristic feature to each local area of skin.

It is difficult to conjecture what skin elements are affected by the rapid induction currents under consideration. They do not produce pressure in the ordinary sense; so far as I can make out, they do not produce sensations of temperature; and the sensations are not of a painful character. They may act simply on the delicate sensory nerve filaments everywhere abounding, and they give rise to sensations of a peculiar kind, a sense corresponding to electrical stimulation and to nothing else.

So far as I am aware, no observations have been made on the stimulation of sensory skin nerves except with reference to the production of reflex acts. The effects of stimuli, varying in number, in intensity, in rhythm on consciousness, have not been studied. It is well known that a single stimulus applied to a motor nerve will cause a single twitch, and the characters of a

* G. S. Stanley Hall and H. H. Donaldson, "Motor sensations in skin," *Mind*, October 1885.

twitch, both in its motor and in its electrical aspects, have been carefully investigated. Further, it is known that if the stimuli come after each other with sufficient rapidity, a tetanic state is produced. The duration of the stimulus capable of exciting a muscular contraction has also been noted. Thus Koenig, a pupil of Helmholtz, found that weak induction currents lasting less than ·001 sec. produced no effect, but if the duration of the current was lengthened to ·017 to ·018 sec. a contraction resulted. This observation, of which I have doubts, leads us to suppose that a current must last, when applied to a frog's sciatic nerve, at least one-thousandth of a second to produce an effect on the muscles supplied by the nerve.

It is of interest to inquire into the action of stimuli on sensory nerves. This can readily be done. I introduced into the circuit of the primary various interrupters (clock, metronomes, vibrating springs, tuning forks), varying in speed from 1 to 200 vibrations per second, and the effect on the skin was noted when the fingers were dipped into the vessels containing salt solution in which were immersed the platinum terminals of the secondary. It was easy to discriminate successive stimuli up to about 40 to 50 per second; between 50 and 120 the sensation was a thrill something comparable to the effect of very rapid musical beats causing disagreeable roughness; from 120 up to 200 the effect was of a more solid, continuous character, and was the (to me) pleasurable thrill produced by a series of weak induction shocks. The sensation of continuity of stimulation is analogous to tetanus, although it by no means follows that the molecular changes occurring in a muscle during tetanus are in the least like those happening in a nerve centre which is the seat and material substratum of the sensation.

As I have said, it is an undoubted fact that the sensory nerves of the skin can appreciate electrical stimulations corresponding in number, rhythm, and intensity to the notes or chords of a complicated piece of music. But the sensations experienced when we listen to music are very complicated. They may, however, be subdivided into various elements. There is pitch of individual tones, duration of individual tones, intensity of tone, quality of tone, and, in addition, the relationship of tones, or tonality. Again, we comprehend music in proportion to its rhythm. If it is unrhythmic it is incoherent; but if it is rhythmic, if there is a systematic

grouping of notes with regard to their duration, it gives pleasure. Rhythm has been well called the metre of music. It depends largely on appreciation of time and accent.

The stimulation of the skin by the method devised can therefore only give certain of the elements of music. It gives no accurate appreciation of pitch, although when stimulations are comparatively few in number, the sensation is rough and different from the continuous sensation when the stimulations are very numerous. Quality is absent; nor is there any feeling like that which leads us, when we intelligently listen to music, to be, as it were, searching for the tones that determine scales. There is also absent that effort at analysis which, it appears to me, constitutes one of the pleasures excited by modern music, both as regards its elaborate compositions and the large and complicated instruments and orchestras by which the ideas of the composer are given to the audience. There still remain the elements of rhythm, and this includes both duration of successive stimulations and intensity. As to time and duration, the sensations are quite distinct. I have tested many varieties of both simple and compound times, as known to the musician, and there is no difficulty in distinguishing the one from the other.

In this way, the deaf can be led to understand some of the elements of music. It will, however, if I may use the expression, be music on one plane. Several deaf persons on whom I have experimented have appeared to be startled with the new sensations, and when it was explained that there was something of this in music, they apparently had pleasure. It is also conceivable that if spoken words fell on the microphone-transmitter, sensations of electrical stimulation of the fingers might be explained, agreed upon, and afterwards understood by the deaf.

There is still another aspect of the matter I venture to bring before you. It is known to physiological psychologists that the connections that exist between processes in the brain centres corresponding to the different senses are real physiological connections and not merely intellectual relations. Sensations of sound affect those of colour and *vice versâ*, so vividly in some persons that they always associate colour and sound. Thus Joachim Raff, an eminent musical composer, said that he saw the colour of the flute

to be blue, the hautboy yellow, and the cornet green. Again, it is well known that if an organ of sense be stimulated with a feeble stimulus, and if another organ of sense be simultaneously stimulated, the intensity of the sensation in the first organ is increased. Thus Urbantschitsch* found that sounding a tuning fork may cause a colour, scarcely perceptible at a distance, to be seen more vividly. Smells, tastes, touches may influence sounds; a loud sound may appear less loud when the eyes are closed; sight excites the acuteness of the sense of taste; smell affects sounds; and tastes influence colours. A brilliant illumination of the skin increases sensitiveness to temperature, and a stimulus of heat or cold on one area of skin may affect the tactile sensibility of the other. By electrically irritating the skin with irritations that are not disagreeable but pleasurable, with irritations that come in proper order, duration, time, and rhythm, may we not send nervous impulses to the cerebral centres of the skin which may radiate to those of the ear and thus excite processes resulting in something like the consciousness of sound and music? Possibly, also, in those who once listened to music and have become deaf, electrical stimulations of the skin may awaken a kind of cerebral music, long forgotten, but still capable of giving pleasure.

Demonstration of an improved Phonograph Recorder, and Remarks on the Curves thereby obtained.

Since my last communication on this subject to the Society, in February, my attention has been directed to perfecting a mechanism for obtaining a record of the vibrations imprinted on the wax cylinder of the phonograph, and I wish to acknowledge the great assistance derived from the ingenuity and mechanical skill of Mr Reid and Mr Kean, of the well-known firm in Glasgow known by the name of "James White." The instrument now before you, which I shall call a phonograph recorder, traces out, on a large scale, the curves of the indentations on the wax cylinder corresponding to each vibration of sound, and it does so in a way that seems to be highly satisfactory. It is now an instrument that can be used by other workers in this difficult department of research, and I hope that some of the younger physiologists and physicists will

* *Pflüger's Archiv.*, xliii. 3–4, 1888.

take the matter up and apply it to an extent that, with numerous avocations and with calls to other branches of physiological research, I can never hope to overtake.

The essence of the method consists in (1) driving at a very slow rate the mandrill carrying the wax cylinder of the phonograph on which a record has been taken. It is well to use well-marked records, and, if possible, those taken specially for purposes of investigation, at the time carefully noting the speed of the phonograph. The best speed I have found to be as nearly as possible that of one revolution in half a second. I now drive the mandrill *a*, as shown on the plate, by means of a small electric motor *b*, worked with one or two Obach cells, *b′* of the Q type, and the speed of revolution is one revolution in eight minutes, or nearly 1000 times slower than the speed at which the mandrill rotates when the phonograph is giving out tones. This slow speed gets rid of the inertia of the recording lever now to be described.

On the ring bearing the apparatus for holding the glass diaphragm, which has been removed, there is attached a light lever, cc^{I} in plate, made of aluminium, rectangular in section, and so placed as to have the narrow borders above and below. This lever has its fulcrum in a well-made axle and pivot joint, c^{III} in plate. It is continued backwards, as seen in plate, so that a small weight may, if necessary, be used as an equipoise. About 5 mm. from the pivot end of the lever, there is passing off from it, at right angles, an upright rod, c^{IV}, shod on its lower end with the usual sapphire point of the reproducing phonograph. When this point rests on the wax cylinder, in the groove, and the cylinder moves slowly, as already described, it will be evident that this lever will move up and down, as the recording point slowly moves over the elevations and depressions on the cylinder, and that as the long arm of the lever, cc^{II} in plate, is 205 mm. in length, the amplification at the point of the lever will be considerable.

To secure still greater amplification, the point of this aluminium lever, which we will call the *first lever*, rests on a horizontal stiff rod, *d*, 80 mm. in length, which is attached transversely to the short arm of another lever, to be termed the *second lever*. The fulcrum of this second lever is also a well-made axle and pivot arrangement; the short arm *e* (25 mm.) is horizontal, while the long arm *f*

(110 mm.) is vertical. In this way the movements of the first lever are communicated to the second lever, and they are thus amplified a second time.

From the point of the second lever a thin rigid wire, *g*, passes transversely 80 mm., to be attached to a thin slip of brass g^{I} bearing a fine glass syphon, *m*, like the syphon used in the well-known syphon recorder of Lord Kelvin. This strip of brass bearing the syphon is attached to an upright rod, *h*, bearing a circular weight at its lower end, *k*, delicately pivoted above and below, and having a fine watch spring attached to its upper end, *i*. The syphon is placed horizontally, with the longer limb, *m*, towards the front of the apparatus and with the shorter limb, *l*, dipping into an inkpot, *n*. The ink used is a filtered solution of an aniline colour. It will be evident, on looking at the plate, that any movement of the second lever is thus communicated to the syphon and that oscillations of the syphon are controlled by the weight and the spring already described. The amplification of the movement is also greatly increased.

The paper on which a tracing is taken is rolled out below the long end of the syphon by an ingenious electro-motor arrangement recently devised for Lord Kelvin's syphon recorder. It will be remembered that in the usual form of Lord Kelvin's syphon recorder the ink, electrified by the well-known "mouse-mill," spurts out on the band of paper, o^{III}, and thus a line is recorded in a series of dots and without friction. The new arrangement gets rid of the mouse-mill. The mechanism, oo^{II} (electro-magnetic), which draws the paper forwards at the same time vibrates up and down through a very short distance, and thus, in short intervals of time, brings the paper against the minute drop of ink at the end of the syphon. Thus the end of the syphon does not rub on the paper and there is practically no friction. The tracing appears as a line formed of a number of minute dots, the distances between which correspond to the rate of vibration of the apparatus.

The apparatus is worked by a storage cell, *p*, of about 6–8 volts, and it runs at such a speed that 20 feet of paper are rolled out during the time of one revolution of the phonograph. Consequently, on a length of 20 feet of paper we obtain a record of all the vibrations that were recorded on $7\frac{7}{8}$th inch of surface of

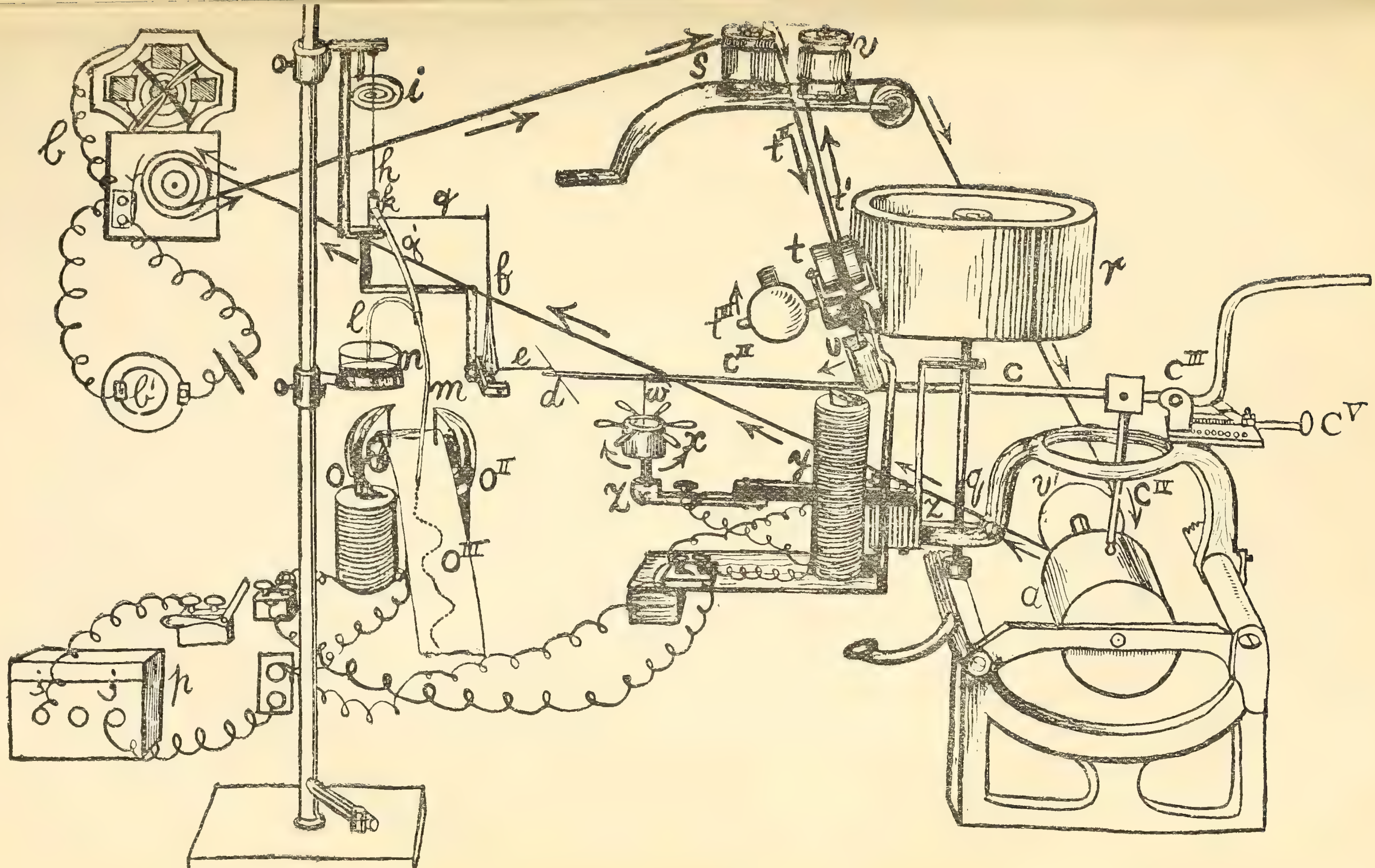


Diagram of the Phonograph Recorder. By Dr William Snodgrass.

cylinder (the circumference of the cylinder), or in a period of half a second. One foot of paper would therefore represent $\frac{1}{40}$th second, and 1 inch would represent $\frac{1}{480}$ of a second.

The amplification vertically is nearly 1000 times, but in the direction of length it is only about thirty-five times magnified. To obtain the tracings so as to increase the amplification in length to correspond to the amplification in height, that is to say, to 1000 times, it would be necessary either to drive the phonograph thirty-six times slower, or a speed of one revolution in about five hours, or to roll out the paper thirty-six times faster, or about 864 feet in eight minutes instead of about 24 feet. Manifestly, either of these devices would be very inconvenient, and it is unnecessary to attempt to carry them out, as for purposes of analysis it does not matter. In the description of the curves of phonographic records taken by Hermann, nothing is said in explanation of the fact that the tracings are not amplifications to scale of the true marks on the phonograph cylinder. I show you here (1) a tracing of the vibrations of a bugle taken by my apparatus, giving an amplification about 1000 times vertically and thirty-five times in a linear direction, and (2) a tracing of a few of these curves enlarged longitudinally so as to have the vertical and longitudinal amplifications equal—about 1000 times. It will be observed how the impressions are long shallow depressions, very shallow at first, gradually becoming deeper towards the centre, and again becoming shallow towards the other end. In this aspect they exactly resemble the grooves seen in photographs taken of portions of the surface of the wax cylinder.

One of the chief difficulties in earlier experiments with this instrument was to secure that the point of the first lever always was in contact with the horizontal portion of the short arm of the second lever, and that, on the other hand, the pressure was not too great. If the mechanism be considered, it will be apparent that, supposing the groove on the wax cylinder to deepen, as would be the case if the sound recorded became more intense, the whole of the aluminium lever would fall and its point would press too heavily on the arm of the second lever. On the other hand, suppose the groove to become shallow, as happens when the sound is less intense, the aluminium lever would rise, and, as it is rigid, its point

might come off the horizontal arm of the second lever. In the first case, when the pressure was too great, the curves recorded on the paper would be less in height than they ought to be, and, if the point of the lever came off the horizontal arm of the second lever, or touched it with a series of knocks, either no curves at all were obtained or the curves were not transcripts of the marks on the cylinder.

I endeavoured to meet this difficulty by turning the screw *q*, on plate, which raises or depresses the arm, bearing, in the usual phonographic arrangement, the reproducing diaphragm, or, as in my instrument, the aluminium lever. Still it was difficult to do this accurately. At last the difficulty was got over by an automatic device. On the top of the screw for raising or elevating the aluminium lever was fixed a round shallow drum, *r*, 80 m. in diameter, and covered, on its outer surface, with sand-paper so as to be rough. Then the cord passing from the electro-motor to the phonograph, for driving slowly the mandrill bearing the wax cylinder, c^{IV}, was brought round a pulley, *s*, and then transversely in front of the side of the roughened drum, *r*. The cord then passed round another pulley, *t*, attached to the keeper of an electro-magnet, marked *v* in the plate, and then back to another pulley, passing transversely to the roughened drum and parallel to the first cord. It then passed round a second pulley, marked *v* on plate, then round the pulley on the phonograph, and back to the electro-motor. When the electro-motor worked, the cord moved slowly in front of the roughened drum and one portion moved in one direction while the other moved in the reverse direction. If the lower cord, t^{I}, touched the roughened drum, it would be moved, carrying the screw with it, in the direction say of the hands of a watch, but if the upper cord, t^{II}, touched the side of the roughened drum, then the direction of the rotation of the screw would be in the opposite direction. It will be evident that the one movement would lower the aluminium lever while the other would raise it.

In the next place, to make the apparatus work automatically, a slender platinum wire, *w*, was attached to the aluminium lever, about 15 mm. from its point. This was caused to dip into a little trough, *x*, containing mercury resting on a horizontal metal arm passing out from the support bearing the aluminium lever, but immediately

beneath the lever and parallel to it, *zz*. Below the pulley round which the cord passed, there is an electro-magnet, and the pulley is so attached to the keeper of the magnet as to be oblique. When the magnet is not acting, the keeper is free, the lower cord touches the roughened drum, and the latter moves in a given direction. If, however, the magnet acts, the keeper bearing the pulley is pulled to one side so as to bring the upper cord against the roughened drum, and the latter moves in the opposite direction. Thus the movement of the screw by which the aluminium lever may be raised or depressed is regulated by the roughened drum, and this again by the electro-magnet and the two cords. Finally, the electro-magnet is brought into action when the platinum wire on the aluminium lever dips into the mercury. This diverts a portion of the current that drives the paper feeder into the electro-magnet, the latter then acts and the upper cord is brought into play. The following is an account exactly of what occurs :—

1. When the reproducing point connected with the aluminium lever *sinks too low*, the wire attached to the lever dips in the mercury; the current is made, the armature is drawn forwards, the upper cord presses on the rough cylinder, the cylinder with the screw turns in a direction against that of the hands of a watch, the bearer of the lever and the reproducing point are lowered; the reproducing point pressing on the cylinder, therefore, lifts the lever, and *the free end of the lever is raised.* Again

2. When the reproducing point rises too high, the wire attached to the lever is lifted out of the mercury, the current is broken, the armature falls away by the action of a counterpoise, the lower cord presses on the rough cylinder, the cylinder and screw are turned in the direction of the hands of a watch, the bearing apparatus is raised, the recording point on the aluminium lever is relatively lowered, and the *free end of the lever falls.*

By this arrangement, which may be called an automatic "finger and thumb" for turning the screw in reverse directions, there is a constant self adjustment according to the depth of the grooves on the wax cylinder, which, again, corresponds with the varying intensity of the sounds.

The Curves.

Since the apparatus was brought to its present condition, I have been able to record the vibrations of the tones of several instruments and also the tones of the human voice both in singing and in speech, but there has not yet been leisure for a careful scrutiny of the records, and I shall therefore, on the present occasion, offer a few general remarks.

First, with reference to speech, I wish to point out that when the record of a *word* is examined it is found to consist of a long series of waves, the number of which depends (1) on the pitch of the vowel constituents in the word, and (2) on the duration of the whole word or of its syllables individually. There is not for each word a definite wave form, but a vast series of waves, and, even although the greatest care be taken, it is impossible to obtain two records for the same word precisely the same in character. A word is built up of a succession of sounds, all usually of a musical character. Each of these sounds, if taken individually, is represented on the phonograph record by a greater or less number of waves or vibrations, according to the pitch of the sound and its duration. The pitch, of course, will depend on the number of vibrations per second, or per hundredth of a second, according to the standard we take, but the number of the waves counted depends on the duration of the sound. As it is almost impossible to utter the same sound twice over in exactly the same fraction of a second, or in the same interval of time, the number of waves counted varies much in different records. The rate per unit of time determines the pitch, the number the duration of the sound. In a word, these successive sounds blend into each other, and, in many records, the passage from one pitch to another can be distinctly seen. The speech sounds of a man to the ear not consciously engaged in analysis, appear to vary in pitch from 100 to 150 vibrations per second, and the song sounds of a man from 80 to 400 vibrations per second. The sounds that build up a word are chiefly those of the vowels. These give a series of waves, representing a variation in pitch according to the character of the vowel sound. As in the record of a spoken word the pitch is constantly moving up and down, the waves are seen in the record to change in length.

It is also very difficult to notice where one series of waves ends and where another begins. For example, in the word *Con-stan-ti-nople,* the predominant sounds are those of o–a–i–o–ill, and the variation in pitch is observable to the ear if, in *speaking* the word, we allow the sound of the syllables to be prolonged. If we look at the record of the word, we find these variations in pitch indicated by the rate of the waves, or, as the eye may catch this more easily, by the greater or less length of wave according to the pitch of the sound. The consonantal sounds of the word are breaks, as it were, in the stream of air issuing from the oral cavity, and these breaks, owing to labial, dental, buccal, or glossal vibrations, produce sounds that have also often the musical character of vowels. Thus at the beginning of "Constantinople" we have, as will be observed on pronouncing the syllable very slowly, the sound *ŭkkō.* This sound is represented in the record by a series of waves. Then follow the waves of the vowel *ō.* Next we have the sound $\overline{nn}$ (driving the air through the nose), also represented by a series of waves. Next the hissing sound $\overline{iss}$, which has first something in it of the vowel *e* or *i,* and then the *ss-s.* This sound also is shown by a series of waves. Then there is *tă,* which has a double series of waves, (1) those for *it,* or *ť,* and the next for *ă.* This passes into the prolonged vowel *ā,* this into *nn,* this followed by *ti* passing into the vowel *ī,* then another *nn,* then a long *ō,* then a sound like *opp,* and lastly the sound *ill,* a sort of double vowel sound. As so many of these sounds have the characters of vowels, it is impossible by an inspection of the record to say where one set of waves begins and another ends. There are no breaks corresponding to the consonants; the vibrations of the consonants glide on as smoothly as those of the vowels. The *number* of waves producing a word is sometimes enormous. In "Constantinople" there may be 500, or 600, or 800 vibrations. A record of the words "Royal Society of Edinburgh," spoken with the slowness of ordinary speech, showed over 3000 vibrations, and I am not sure if they were all counted. This brief illustration gives one an insight into nature's method of producing speech sounds, and it shows clearly that we can never hope to read such records in the sense of identifying the curve by an inspection of the vibrations. The details are too minute to be of service to us, and we must again fall back

on the power the ear possesses of identifying the sounds, and on the use of conventional signs or symbols, such as letters of the alphabet, vowel symbols, consonant symbols, or the symbols of Chinese, which are monosyllabic roots often meaning very different things according to the inflection of tone, the variations in pitch being used in that language to convey shades of meaning.

When human voice sounds are produced in singing, especially when an open vowel sound is sung on a note of definite pitch, the record is much more easily understood. Then we have the waves following each other with great regularity, and the pitch can easily be made out. Still, as has been well pointed out by Dr R. J. Lloyd of Liverpool, a gentleman who has devoted much time and learning to this subject,* it is impossible by a visual inspection of the vowel curve to recognise its elements. Thus two curves, very similar, possibly identical to the eye, may give different sounds to the ear, that is to say, the ear, or ear and brain together, have analytic powers of the finest delicacy. No doubt, by the application of the Fourierian analysis, we may split up the periodic wave into a fundamental of the same period and a series of waves of varying intensity vibrating 2, 3, 4, 5, &c., times faster than the fundamental, and the relative amplitude of each of these may also be determined. If all these waves of given amplitude and given phase acted simultaneously on a given particle, the particle would describe the original curve. Dr Lloyd, however, is of opinion that even a Fourierian analysis may not exhaust the contents of a vowel, as it does not take account of inharmonic constituents which may possibly exist. Hermann † and Pipping ‡ have also been investigating the analysis of vowel tones, and their investigations have revealed many difficulties.

* Dr R. J. Lloyd, D.Lit., M.A., *Phonetische Studien*, vol. iv. p. 41; "The Interpretation of the Phonograms of Vowels, and the Genesis of Vowels," *Journal of Anatomy and Physiology*, January 1897. Also communicated to the Physiological Section of the British Association, Liverpool Meeting, 1896. As one of the editors of the above Journal, I had the privilege of seeing Dr Lloyd's papers in proof.

† Hermann, "Ueber das Verhalten der Vocale am neuen Edisonische Phonographen," *Pflüger's Archiv.*, v. 47, 1890; also "Phonophotographische Untersuchungen," *op. cit.*, ii. and iii.

‡ Pipping, *Om Klangfärgen hos sjungna Vokaler.* Discussed in Dr Lloyd's paper, "On the Interpretation of the Phonograms of Vowels," *op. cit.*

Hermann experimented with the ordinary phonograph, and obtained photographs of the movements of the vibrating glass plate. His curves are small, not unlike those seen in Koenig's flame pictures, and they do not seem to me to represent so accurately the marks on the wax cylinder as those obtained by my apparatus. In many cases they have sharp points. This, however, may not interfere with analysis. Pipping's curves were not obtained from the phonograph but from the vibrations of a minute membrane made to represent the drum head of the ear. His curves show large periodic waves with minute waves on their summits, and they suggest that the large waves may be vibrations due to the membrane itself. Not having seen the apparatus, and as the observations have been made by one well aware of the possibility of this error, I do not venture to do more than suggest this difficulty, especially as I now show you a series of tracings on a glass plate very similar to those in Pipping's figures. These were obtained by singing a vowel into a receiver furnished with a small membrane to which a recorder was attached. The glass plate (smoked) moved rapidly across in front of the marker. Alongside of these you will see curves obtained directly from the recorder attached to the glass disk of a phonograph. In the tracings of another experiment, you see waves more like those of Hermann. The larger waves in the tracing like that of Pipping, are, I believe, due in my experiment to the vibrator and do not represent the glottal vibrations. This conclusion is strengthened by noting the pitch of the sound, as made out by counting not the larger but the smaller waves, which corresponds to that of the sound given to the membrane. I therefore think that argument should be based only on records obtained from the phonograph itself, which is furnished with a vibrator that will not record its own periodic vibrations unless the sound be remarkably intense. In ordinary voice production, and in ordinary singing, the vibrator of the phonograph faithfully records only the pressures falling upon it—no more and no less.

Having made so large a demand on your patience, I shall not say more at present on the curves obtained from musical instruments.

And now I declare the session 1896–97 to be opened. I hope

it will be a session fruitful of good work. Both those who come here to describe the results of research, and those who listen, will feel an intellectual stimulus, and they will have that craving, not only for a knowledge of facts but for a knowledge of causes, which, paradoxically, is at once one of the most satisfying and the most unsatisfying of all human feelings. Having reached one explanation, there is, for the moment, a thrill of satisfaction, but this soon passes off when we contemplate new problems and new difficulties to be overcome.

Postscript.—With reference to the statement on p. 193 regarding the deaf, Dr M'Kendrick read a communication he had received from Dr James Kerr Love, Glasgow, regarding the results of an experiment with four deaf-mutes, two boys and two girls, made in the Physiological Laboratory of the University of Glasgow, on 5th December. The children came from the Deaf and Dumb Institution of Glasgow, and they were accompanied by Dr Love, who takes a great interest in deaf mutes, and by Mr Welch and Mr Haycock, both masters in the institution. Dr M'Kendrick carried out the experiment. One girl, aged 17, who has been quite deaf since she was 11 years of age, said that the sensation she felt when the fingers were immersed in the salt solution was *music*, and that the feeling recalled something to her memory. She had accurate perception of the rhythm of the music played by the phonograph. The other three also experienced rhythm, and nodded their heads in time to the music. A young lady, deaf from her birth, examined by Dr M'Kendrick on 4th December, wrote that she had a sensation like the purring of a cat, with the "purrs" long and short, and strong and weak. She also compared it to the sensation of having a musical box in her hand.

MEASUREMENTS OF THE RECORDING APPARATUS.

First Lever.

1. Length of upright, shod with sapphire, 35 mm.
2. Distance of power from fulcrum, 5 mm.
3. Length of lever from power, 205 mm. (Aluminium.)

Second Lever.

4. Length of short arm, 25 mm.
5. Length of long arm, 110 mm.

Third Lever.

6. Length of short arm, 6 mm.
7. Length of long arm, 95 mm.
8. Length of upright carrying syphon, 75 mm.
9. Length of weight, 25 mm.
10. Diameter of weight, 10 mm.
11. Length of screw, 130 mm.
12. Diameter of drum with roughened sides on head of screw, 80 mm.
13. Distance of wire from point of aluminium lever, 35 mm.
14. Length of fine wire dipping into mercury trough, 16 mm.
15. Length of support of Hg trough, 130 mm.
16. Diameter of Hg trough, 15 mm.
17. Length of electro-magnet of paper apparatus, 85 mm.; also of adjuster, 85 mm.
18. Distance of adjuster pulley to two pulleys, 250 mm.
19. Distance from centre of band of recording paper to adjuster, 208 mm.
20. Distance from centre of pulley driving phono-cylinder or mandrill to centre of nearest pulley, 130 mm.
21. Distance of electro-motor to nearest pulley, 720 mm.
22. Height of electro-motor, 170 mm.
23. Diameter of electro-motor, 110 mm.
24. Diameter of pulley on electro-motor, 40 mm.
25. Diameter of pulley on syphon, 55 mm.
26. Height of rod, 400 mm.
27. Length of short arm of syphon, 40 mm.
28. Length of long arm, 80 mm.
29. Length of transverse wire of second lever, 80 mm.

On the Reproduction of some Marine Diatoms. By George Murray, F.R.S.E., F.L.S., Keeper of Botany, British Museum. (With Three Plates.)

(Read December 7, 1896.)

The subject of the production within Diatoms of spores, endocysts, &c., has been worked at by several observers, principally Castracane * in recent years, and before him O'Meara † and Rabenhorst.‡ Some of these observers have noted ciliated, spore-like bodies, probably parasitic or casual intrusive organisms; others, notably Castracane, have recorded oval and other cysts which have not been figured and the characters of which are difficult to realise. Miquel § has made an experimental examination of the subject without being able to confirm Castracane's results. Castracane || has also recorded the presence, as he thinks, of "gonids or embryonal forms" within a fossil Diatom (*Coscinodiscus punctatus*) in a marine deposit of miocene age. During March of this year I tow-netted near the Bell Rock a large quantity of *Coscinodiscus concinnus* — with many dead valves separated at the girdle. These frequently contained not only other and smaller individuals of the same species, but other species of diatoms and casual objects as well, and the observation makes one cautious about accepting evidence of the character brought forward by Castracane as to his fossil form.

Lauder,¶ in describing forms of *Bacteriastrum* and *Chætoceros*, observed the formation of endocysts. "At certain times, or under certain circumstances, the endochrome does not divide after the

* *Le Diatomiste*, ii. (1893); *Atti Accad. Pontif. Nuovi Lincei*, xlvii. (1894), French ed. in *Le Diatomiste*, ii. (1894); *Atti Accad. Pontif. Nuovi Lincei*, xlviii. (1895).

† "Anthozoids in *Pleurosigma Spenceri*," *Proc. Dubl. Soc. Nat. Hist.* (1856) and *Quart. Journ. Micr. Sci.* (1858).

‡ Conf. Carpenter on the *Microscope*, ed. vii., by Dallinger, 1891, p. 526.

§ *Le Diatomiste*, ii. (1893).

|| *Atti Accad. Pontif. Nuovi Lincei* (1885).

¶ *Trans. Micr. Soc.*, vol. xii., n. s., 1864, pp. 7 and 75.

lengthening of the frustule, but secretes a siliceous envelope and becomes a gonidium or sporangium, consisting of a cell with two rounded ends, and a connecting hoop, one end being smaller than the other. . . . . I may mention here that these sporangia (if one may call them so) of *Bacteriastrum* and *Chætoceros* are very variable in form and size, and, from having been found in a free state in deposit or guano, have been very often mistaken for distinct species of diatoms by many observers." Again he says (p. 76), after describing the ordinary division in *Chætoceros*, "The condensed endochrome, instead of becoming paler and dividing, gradually assumes another shape, varying with the species, and secretes a siliceous envelope. . . . . The contents of the sporangium soon escape, but I have not been able to follow out the further processes they undergo towards the reproduction of a *Chætoceros.*"

Castracane, in describing the "Challenger" diatoms, found a form known as *Dicladia capreolus* within the frustules of *Chætoceros*, and Mr Thomas Comber * has quite recently described the endocysts of *Thalassiosira antarctica.* It appears to be established that in these genera there occurs a kind of dimorphism, possibly an alternation of forms hitherto believed to belong to different genera.

Finally, Professor Cleve † has figured, in his *Diatoms of the Arctic Sea*, one *Biddulphia aurita* within another. He observes respecting it: "In one specimen from Greenland I have observed a sporangial frustule. The large exterior cell measured 0·066 mm. in height, and 0·044 mm. in breadth, and the included cell was about half the size of the exterior, which it resembled in all its characters." In the figure, however, the spines of the parent form are not shown on the offspring. I have repeated one of Professor Cleve's figures (Plate I. fig. 1) on account of the interest it adds to my own observations.

Many species of diatoms have been recorded with "internal valves" as they are called, and it is generally supposed that they represent resting stages—or a provision against drying up—corresponding to a thickening of the membrane in ordinary cells. This,

* *Journ. Roy. Micr. Soc.*, October 1896.

† *Bihang till K. Svenska Vet. Akad. Handlingar*, Bd. I. No. 13, p. 9, tab. 1, fig. 3, *a* and *b*.

however, is a different condition from the one described above, though it has an interest in connection with it.

For several years Dr John Murray has urged me to investigate the pelagic Algæ, and in March of this year I gratefully accepted his invitation to make a study of the marine diatoms on behalf of the Fishery Board for Scotland, and my special thanks are due to the Board and to Dr Wemyss Fulton, the Superintendent of Scientific Investigations, for the facilities freely accorded me for the prosecution of this research. I spent a fortnight in March and April and three weeks in August on the fishery cruiser "Garland," engaged in this work principally on the west coast of Scotland. The ordinary method of capture by means of fine silk tow-nets was employed, but Dr Murray's ingenuity furnished me with another resource for employment in rough weather, when tow-netting is impossible or at least attended with risk to the nets; and the conditions of sea climate for part of last March and April made me mindful of his foresight. This instrument was a fine silk bag, shaped somewhat like a large German sausage, to be fastened over the nozzle of the hose used for washing the decks. The bag had a small lateral overflow vent about the size of a sixpence (which it is well to strengthen with an extra thickness of silk and a "buttonhole stitch"), and it is useful in rough weather or after dark. If the donkey-engine is made to pump through it gently for half an hour, under most circumstances a good haul is obtained. The nozzle and bag should be fastened to a stanchion or boat davit above the level of the bulwark, where it is safe from injury by the sea. While describing methods, I may record the entire success of the fixing and preservative fluid employed, the precise strength of which was exactly forecasted by my colleague, Mr V. H. Blackman. I carried a number of strong tubes half filled with 0·5 per cent. aqueous solution of chromic acid. This is about the same density as ordinary sea-water, and I simply added my captures in sea-water to it, filling up the tube. If this be done with care the cell-contents are fixed and maintain the appearance they possessed before death. During the autumn I spoiled several tubes by adding too dense a mass of diatoms, displacing the proportion of sea-water, and it sufficed to ruin the condition of the cell-contents in these cases. The process must therefore be carried out

with care, and I warn collectors against removing spoonfuls of diatoms direct from the tow-net to the tube. It is better to float the whole capture in a glass jar, to permit the whole to settle for an hour or so, and then to collect from the bottom by means of a dipping tube. This method gives, without special calculation, the right proportions of diatoms and sea-water to be added to the half-filled tube of chromic acid solution. With such an outfit of tubes secured in a rack-work box, and by clamping the microscope down to a bench, it is possible to continue work with all powers but the immersion lenses even in moderately bad weather. The use of such high objectives as are now commonly immersed is too fatiguing to the eyes while the body is swaying in different time to the vessel, from the difficulty of closely accommodating the eye to the ocular for specially minute study requiring an uninterrupted stare into the field.

During the first months of the year, as Dr John Murray has recorded, there is in all our seas an extraordinary prevalence of diatom life, and I found this to be specially true of the west coast and Clyde sea area. It is only necessary to put down the net for a few minutes to obtain at this season a dense mass of diatoms which nearly half fills the net with a yellow-brown scum of the consistency of soft soap. Sir Joseph Hooker (on Sir James Ross's Antarctic Expedition) and the naturalists of the "Challenger" Expedition have made us familiar with the extraordinary abundance of diatoms in the Southern Ocean. South of latitude 50° the tow-nets of the "Challenger" Expedition were sometimes so filled with diatom scum "that large quantities could be dried by heating over a stove, when a whitish felt-like mass was obtained." It is not necessary to visit the Southern Ocean to see this impressive manifestation of diatom life in the sea. I dried similar masses over the boilers of the "Garland" in the Clyde sea area last April, and the prevalent form was in all cases *Skeletonema costatum* with a slight mixture of *Coscinodiscus concinnus*, *Chætoceros borealis*, and other forms. This vast occurrence of diatoms is a seasonal phenomenon, and they become scarcer in summer (except here and there in particular shoals), and are largely replaced by species of *Ceratium*—notably *C. Tripos*, *C. fuscum*, and *C. Fusus*. For example, the prevalent *Skeletonema* of April had disappeared in

summer, so far as my observations went, from the west coast except in two places, viz., off Sheep Island near Oban, where it occurred sparingly, and in Loch Etive, where it occurred in almost as great abundance as in the Clyde in spring. I could find no local temperature conditions to account for this—the only outstanding physical point of resemblance was the large quantity of fresh water on the surface of the Clyde in spring and in Loch Etive in August. Only in Loch Etive and in the Sound of Mull did I obtain hauls of diatoms in summer that were comparable in quantity with those obtained generally in the sea in spring. This seasonal occurrence may be connected, I venture to think, with another observation of economic interest. I discovered that the Copepoda and other minute crustacea were living on the diatoms, both by direct observation and by examining their cylindrical lumps of excrement, which consisted entirely of diatom chromatophores (maintaining perfectly their characteristic shapes and even partly their hue) and of minute fragments of the siliceous frustules—both chromatophores and fragments of frustules being determinable as those of the diatoms in the same haul. I searched daily for three weeks in summer for similar evidence of their eating *Ceratium*, but in vain. This negative evidence, however, is not of much value, since the *Peridinieæ*, from their structure, would be less likely to leave traces after passage through the digestive tract. Every diatomist knows of the abundance of diatoms in the guano deposits of sea birds that presumably devoured fishes that in turn live on small crustacea. Moreover, diatoms are frequently found in the stomachs of Holothuriæ, Ascidians, Salpæ; oysters, scallops, whelks, and other molluscs; crab, lobster, and other large crustacea, and even in those of full grown fishes. We have been able in the past to infer that diatoms and other pelagic algæ formed the basis of the nutrition of all sea animals,—the pastures of the sea,—and I count myself fortunate to have been able to establish by direct observation this important link—that the small crustacea, which themselves are so important a food of fishes, feed directly on diatoms.

In order to determine whether diatoms are present to any extent in the digestive tract of young fishes, either from having been eaten directly by them or inside small crustacea eaten by the fish,

Dr Wemyss Fulton sent me some young sand-eels, taken in tow-net, 15 miles off Aberdeen on 16th May 1894, and preserved in spirit, some young flat fish (plaice?) taken off Montrose 21st May 1894, and some very small Clupeoid fishes taken 30th March 1889. In all of them diatoms were present. In the sand-eels four genera, *Skeletonema*, *Eucampia*, *Melosira*, and *Chætoceros*; in the flat fish *Skeletonema* and *Nitzschia* (in both cases *Skeletonema* being predominant), and in the Clupeoids *Coscinodiscus* (abundant) and *Melosira*. All these were complete valves, which appears to indicate that they were eaten directly by the fish, and not within minute crustacea, in which case they would have been broken up into fragments.

The seasonal occurrence of diatoms has, however, its special botanical interest. The ordinary processes of reproduction by simple division and the more rare formation of auxospores does not appear from my opening citation of literature to have satisfied observers. Observations, accurate or not, of other forms of reproduction have been in the air, if the expression may be permitted, for some years, and Dr John Murray has, with rare divination, always associated this seasonal occurrence with some mode of reproduction yet to be disclosed. At his instigation, then, I have made the following observations, and since the modes of reproduction are of diverse sorts, though all broadly the same, I will proceed from the simplest case to the others.

In a tow-net haul obtained last April off Gigha there was a large quantity of *Biddulphia mobiliensis*. Many of the specimens were in the state represented in Plate I. fig. 2; the whole of the cell-contents rounded off and in many cases with a large oil drop enclosed by the chromatophores, nucleus, &c. The same was the case with *Ditylum Brightwellii*, and an extreme form of contraction of contents in this species is figured on the same plate (fig. 6). It was only on a subsequent and more detailed examination of this and other captures that the state represented on fig. 3 was discovered. There is here within the parent *Biddulphia* a young "cyst" with a slightly silicified wall, but without the characteristic spines, &c., of the parent form. I take this "cyst" of *B. mobiliensis* to correspond to the form described by Professor Cleve (reproduced in fig. 1) for *Biddulphia aurita* (which also is without

the fine spines of the parent), and the stages of contraction, &c., of the cell-contents to be precedent to its development.

These agree well enough with the "condensed endochrome" of Lauder, quoted above in speaking of *Bacteriastrum* and *Chætoceros.* These bodies interested me much at the time, ignorant as I then was of any observations of the occurrence of endocysts in diatoms, and I took the process (as indeed it is) to be a form of rejuvenescence of the diatom cell. I have fortunately been able to throw some light on their subsequent history. In August I found the same bodies extremely plentiful on the west coast, outside the Clyde sea area, from the Sound of Islay to Loch Hourn. They were mostly a good deal longer (fig. 4) and frequently in a state of division (fig. 5). They exactly resembled in every botanical character the young "cysts" of spring, and there was at the same time an almost entire absence of the characteristic form of *Biddulphia mobiliensis.* It appears, then, that these reproductive bodies possess the power of dividing and multiplying to a great extent before they assume the characteristic parent form of *B. mobiliensis.* From their abundance I should not be surprised to hear that they have been described under a separate specific name.

[*Note.*—During the first week of December the characteristic form of *Biddulphia mobiliensis* was abundant on the west coast, and the young forms observed in August were not to be found. Presumably they had developed in the meantime into the mature forms.—10th December 1896.]

On the same plate (fig. 7) there is represented a specimen of *Coscinodiscus concinnus* with another inside it. At first I took this (and others of the kind) to be merely accidental intrusions—the more readily that I had observed a large number of such intrusions in a rich haul of *Coscinodiscus* near the Bell Rock.

After turning the specimen over and over many times, I was, however, forced to the conclusion that there was here a young *Coscinodiscus* veritably within another. I subsequently met with many such, especially in Loch Fyne in spring, and I naturally put it down, as in *Biddulphia,* to a rejuvenescence of the cell. Among many such examples I found the form figured on Plate II. figs. 1, *a* and 1, *b*. It puzzled me greatly to account for it. We have here within the parent form two young diatoms at first sight singularly

unlike each other. Here, again, it was only after many turnings over of the specimen that I was able to convince myself (especially after seeing it as in 1, *b*) that these two forms were actually within the intact parent frustule. The larger of the two young ones has a very wide girdle-zone, and is apparently distended in this direction to the utmost capacity of the girdle, giving it a drum-shape. The other, which in both figures presents the girdle-view (having moved within the parent), shows a girdle indeed of a scarcely perceptible breadth, and apparently so tightly shut up that the rounded ends of the diatom practically meet each other at the girdle, and the figure of the whole is oval rather than drum-shaped with convex ends. Both have numerous chromatophores, of the usual shape in *Coscinodiscus concinnus*, but arranged in stars. This specimen, however, interested me particularly, as showing that more than one so-called "cyst" could be produced at a time,—that it was not always a simple rejuvenescence of the whole cell-contents, that the protoplasm, in short, sometimes divided before the production of these bodies.

While occupied with the study of these forms last April, I was much struck by the abundance in the Clyde sea area, and especially in Loch Fyne, of minute *Coscinodisci* presenting the characters of *C. concinnus.* They occurred singly, but for the greater part in small packets of eight and of sixteen (Plate II. figs. 4 and 5). Other numbers occurred, *e.g.*, four, but the prevalent number was eight, and less frequently sixteen. I could not help observing that, when there were sixteen, the individuals were approximately half the size of those in packets of eight, and it was the like case with those in the packets of eight and of four (see Plate II. figs. 4 and 5). They were held together in all cases by a fine membrane represented in the figures. The valve-view was very slightly convex, not nearly so much so as the parent frustule in fig. 1, *b*, more like that of fig. 2, *b*, but even less so, and in the packets of eight the young diatoms, though larger, were considerably thinner (girdle-view) than those of the packets of sixteen. The instances in figs. 4 and 5 are perhaps extreme—but there was considerable variety in the respects mentioned, and yet the packets were all unmistakably of like origin.

While the source of these packets remained a mystery, I took, on

the 6th of April, a haul of the tow-nets simultaneously at the surface, five, twenty-five, and fifty fathoms in the deep water of Loch Fyne between the Otter and Tarbert. This haul was remarkable, not only for the large number of *Coscinodisci* it yielded with one each inside, but the individuals from the surface and five fathoms very frequently had their contents divided into eight and sixteen rounded-off portions, as in figs. 2, *a*, *b*, and 3. Mixed with these were a large number of packets of young *Coscinodisci* and large empty frustules. Even down at twenty-five fathoms the number of packets was almost as great as at the surface and five fathoms. Where the cell-contents were divided into eight, the eight portions were each nearly twice the volume of those in the cells which had divided into sixteen. At first I naturally jumped to the conclusion that the eight and sixteen rounded-off portions were the outcome of free cell-formation, but a more minute study has convinced me that they are produced by successive divisions into two. The relative positions of the portions as shown in fig. 2, *a*, and even more strikingly in other instances I have seen and photographed, point to this mode of formation. The association of these with the packets, the correspondence in size of the eights and sixteens respectively, the adequacy of these divided portions to the formation of the packets, as indicated by the undivided instance of *Biddulphia mobiliensis*, the occurrence of two young *Coscinodisci* within a parent frustule, all lead irresistibly to our interpreting the divisions of the contents as preliminary to the formation of the packets.

Granting this to be the explanation, I next found myself in face of the difficulty of accounting for the future progress of the young *Coscinodisci*. Diatoms, we are accustomed to be told, go on decreasing in size by the breadth of the girdle membrane at each succeeding ordinary division until a minimum size is reached, when the formation of an auxospore re-establishes them at the maximum. This is true enough, but it is generally associated with the statement that, owing to the rigid silicified nature of their membranes, individual diatoms are incapable of superficial growth. I am by no means sure that this last statement has any greater value than a likely assertion, but if it be true, how then would it fare with these small *Coscinodisci*, launched into the world at what must surely be their minimum size? Would they start life with the formation

of auxospores? This remained a puzzle until when working over, and verifying observations in London, it occurred to me to test the siliceous character of the membranes of these young packets of *Coscinodisci.* Taking samples known to contain plenty of them among other diatoms, and "cleaning" them by burning and by treatment with nitric acid and chlorate of potash, it was found in every case that the packets totally disappeared, while the other diatoms remained. The conclusion, plainly warranted, then is, that the individual membranes of the young packet *Coscinodisci* are either not silicified or are incompletely so, and that they are therefore capable of superficial growth, however it may be with diatoms the walls of which are perfectly silicified.

It appears, then, that these marine diatoms may reproduce themselves, either by a rejuvenescence of the cell and the secretion of a new frustule within the parent (*Biddulphia*, *Coscinodiscus*, and possibly *Ditylum*), which, escaping on the separation of the parent valves at the girdle, may grow, divide and multiply before fully attaining the characteristic external sculpturing and adornment of the parent (*Biddulphia*), or the number of the offspring may be increased by preliminary divisions of the protoplasm into two, four, eight, and sixteen (*Coscinodiscus*).

Both last April and in August I observed in different species of *Chœtoceros* (in addition to the usual cell-division) a series of successive divisions within individual cells which are manifestly preliminary to the formation of reproductive bodies. In the ordinary multiplication of a *Chœtoceros*, the cell of course divides transversely to the axis of the chain of cells. In *Ch. constrictus* (Plate III. fig. 3), when farther subdivisions of the cell-contents are destined to take place, the first division is parallel to the axis as shown in the upper cell. In the lower cell the second division, transverse to the axis, has taken place. In *Ch. curvisetus* (fig. 4) the same thing happens. In both cases a drop or cushion of oil separates the two halves at the first division. The second division (transverse) cuts right through the middle cell of fig. 4, and parts the resulting portions into two pairs, giving them the appearance of having first divided transversely, which however is not the case in these species. In *Ch. borealis* (fig. 1) the first division is always transverse. Though ordinarily all the cells of a chain are approximately at the

same stage of development, not however so regularly in diatoms as in the cells of a filamentous Alga, the remarkable chain represented in fig. 1 shows three successive stages in three adjoining cells. The uppermost cell has undergone the first (transverse) division. In the second cell the contents are divided into four portions, and in the lowest cell each of these is rounded off. I have witnessed the processes of division up to four many times in the living *Chætoceros*, but never the actual process of further subdivision. In fig. 2 there are represented two cells, one with eight, the other with sixteen completely rounded-off portions, and such variation of number in adjoining cells is not uncommon. As said above, I never had the good fortune to witness the subdivisions into eight and sixteen in the living cells, and it was only on searching the preserved material in London that instances came to light. In *Ch. curvisetus* (fig. 5) subdivisions into four and eight are shown also in adjoining cells.

That these spore-like cells of *Chætoceros* are destined to reproduce the parent form no one can doubt. But how? Do they form *Chætoceros* filaments by simple vegetative growth, each first secreting a membrane, siliceous or not, like the so-called "cysts" of *Biddulphia*—or like the eight and sixteen subdivided and rounded-off portions of the *Coscinodiscus* cell? In the absence of trustworthy evidence the inevitable comparison with *Coscinodiscus* appears to furnish the most likely interpretation.

That five weeks' work at sea should have been rewarded with such unlooked for results—results which no one could have anticipated from the known facts about diatoms—I explain by the fact that botanists have hitherto confined their marine studies almost exclusively to shore plants. The instances of a botanist using a tow-net are few indeed, and I can only hope they will be more numerous in the future. The plant plankton of our coasts is exceedingly rich in minute Algæ other than diatoms, the life-histories of which are imperfectly known.

There is one object occasionally met with in the fine silk tow-net in autumn in the lochs of Western Scotland which I may be permitted to mention. In the Sound of Islay and again in Loch Hourn (though to a less extent) the adjoining hills were covered with heather in bloom, and I repeatedly captured quantities of

pollen-grains of this plant in the surface-net—objects which, among unicellular marine Algæ, at first sight puzzled me not a little.

In order to attempt the settlement of various questions that have arisen in the course of this investigation, and especially the tracing of life-histories by continuous observations, I have had two large tanks prepared for the purpose of cultivating marine diatoms, with the hope of overcoming the numerous difficulties that attend such an experiment.

Professor Weiss, of the Owens College, Manchester, has informed me in a letter that he has made observations similar to some of those recorded here. On learning that I was working at the subject, he generously refrained from competition in priority of publication.

In addition to work at sea, in which I have to acknowledge the courtesy and kindness of Captain R. Campbell of the "Garland," it has been necessary to search microscopically and to mount a vast quantity of preserved material, and I desire to record my thanks for cheerful assistance in this task to Miss Barton, Miss Frances Whitting, and Mr Percy Highley, who have all aided me with valuable suggestions and observations. I must thank Professor Cleve for the names of some of the diatoms.

EXPLANATION OF PLATES.

Plate I.

Fig. 1. *Biddulphia aurita*, with young *Biddulphia* within it (after Cleve).
Fig. 2. *B. mobiliensis*, with cell-contents rounded off. × 800.
Fig. 3. Do., with young *Biddulphia* within it. × 800.
Fig. 4. Do., young form free. × 600.
Fig. 5. Do., dividing. × 600.
Fig. 6. *Ditylum Brightwellii*, with contents rounded off. × 800.
Fig. 7. *Coscinodiscus concinnus*, with one young *Coscinodiscus* within it. × 500.

Plate II.

Fig. 1, *a* and *b*. *Coscinodiscus concinnus*, with two frustules within it. × 500.

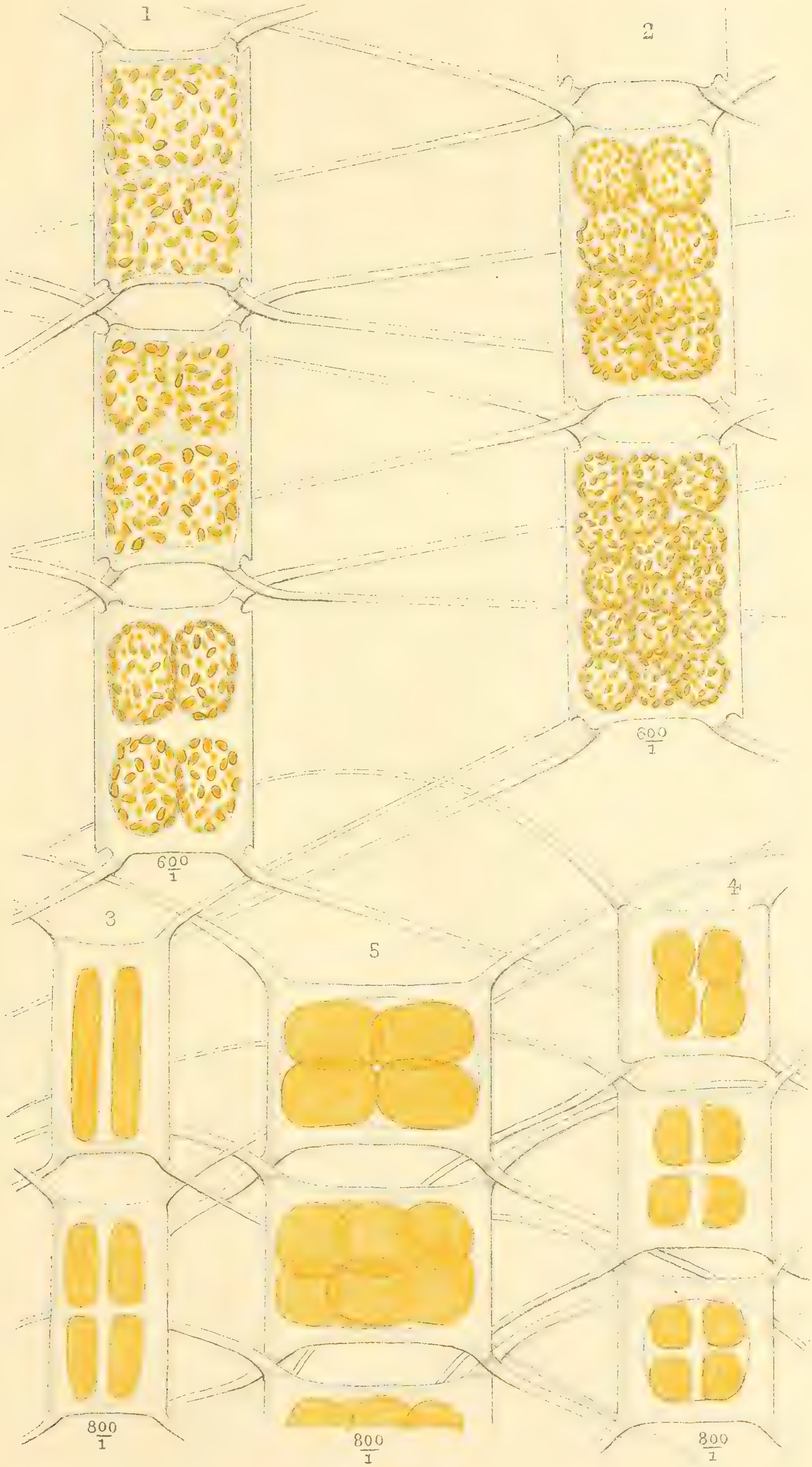
Fig. 2. Do., with contents divided into eight; *a*, valve-view; *b*, girdle-view. × 500.

Highley de. et lith. Hanhart imp.

George Murray on the Reproduction of Marine Diatoms.

Highley del. et lith. Hanhart imp.

George Murray on the Reproduction of Marine Diatoms.



Highley del. et lith.

Hanhart imp.

George Murray on the Reproduction of Marine Diatoms.

Fig. 3. Do., with contents divided into sixteen. × 500.
Fig. 4. Do., packet of eight young *Coscinodisci.* × 500.
Fig. 5. Do., packet of sixteen young *Coscinodisci.* × 500.

PLATE III.

Fig. 1. *Chætoceros borealis,* uppermost cell, dividing transversely ; middle cell into four ; lowest cell, portions rounded off. × 600.

Fig. 2. Do., showing divisions into eight and sixteen. × 600.

Fig. 3. *Ch. constrictus,* upper cell showing first division parallel to axis of chain, lower cell with second (transverse) division. × 800.

Fig. 4. *Ch. curvisetus,* showing division into four. × 800.

Fig. 5. Do., showing divisions into four and eight. × 800.

On the Eliminant of a Set of Ternary Quadrics.

By Thomas Muir, LL.D.

(Read December 7, 1896.)

(1) In the *Cambridge Math. Journal*, ii. p. 233, Sylvester showed how to eliminate x, y, z from the set of equations

$$\left.\begin{array}{l} Ax^2 + ayz + bzx + cxy = 0 \\ My^2 + lyz + mzx + nxy = 0 \\ Rz^2 + pyz + qzx + rxy = 0 \end{array}\right\} .$$

His method consisted in deriving other three equations involving the variables x^2, y^2, z^2, yz, zx, xy, and then eliminating these six variables from the six equations. The result obtained was *

$$\left|\begin{array}{cccccc} A & . & . & a & b & c \\ . & M & . & l & m & n \\ . & . & R & p & q & r \\ . & ra - c(M+p) & ma - b(R+l) & \Phi & -A(R+l) & -A(M+p) \\ mr - n(A+q) & . & ma - l(R+b) & -M(R+b) & X & -M(A+q) \\ mr - q(A+n) & ra - p(M+c) & . & -R(M+c) & -R(A+n) & \Psi \end{array}\right|$$

where
$$\begin{aligned} \Phi &= a(n+q) - b(M+p) - c(l+R), \\ X &= m(p+c) - n(R+b) - l(q+A), \\ \Psi &= r(b+l) - p(A+n) - q(c+M); \end{aligned}$$

—a result which, on account of its complexity, it is impossible to rest satisfied with.

The manifest fact that when $A = M = R = 0$, the eliminant takes the form

$$\left|\begin{array}{ccc} a & b & c \\ l & m & n \\ p & q & r \end{array}\right| ,$$

and the further fact, that when $a = m = r = 0$, the equations become linear, with the eliminant

$$\left|\begin{array}{ccc} A & c & b \\ n & M & l \\ q & p & R \end{array}\right|$$

* There are several troublesome misprints in the original.

raise the presumption that Sylvester had not hit upon the simplest mode of performing the elimination.

(2) Looking to the terms which do not contain x in the first two equations, we see that by multiplying both sides of the first equation by $\mathrm{M}y + lz$, and both sides of the second by az, we shall have, after subtraction, an equation consisting of terms all containing x, and from which this factor may be removed. The result in fact is

$$c\mathrm{M}y^2 + (bl - am)z^2 + (b\mathrm{M} + cl - an)yz + \mathrm{A}lzx + \mathrm{AM}xy = 0$$

—a derived equation which is already simpler than Sylvester's, but which may be further simplified by using the second of the original equations to eliminate the term containing y^2. Doing this we have

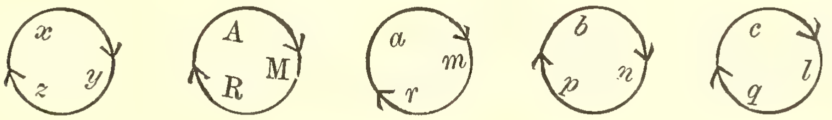
$$(bl - am)z^2 + (b\mathrm{M} - an)yz + (\mathrm{A}l - cm)zx + (\mathrm{AM} - cn)xy = 0.$$

In a similar manner it may be shown that

$$(nq - mr)x^2 + (\mathrm{MR} - lp)yz + (n\mathrm{R} - mp)zx + (\mathrm{M}q - lr)xy = 0,$$

and $$(pc - ra)y^2 + (\mathrm{R}c - qa)yz + (\mathrm{RA} - qb)zx + (p\mathrm{A} - rb)xy = 0;$$

but, having obtained one equation, we may obtain the two others of like kind by merely changing the letters in accordance with the cycles



this being the mode in which any one of the original set is obtained from one of the remaining two.

(3) Another mode of obtaining the equations of the preceding paragraph may be noted in passing. It is much simpler, although less likely to occur to one at first. Writing the first two equations of the original set in the form

$$\left.\begin{aligned}(\mathrm{A}x + bz)x + (az + cx)y = 0 \\ (ny + mz)x + (\mathrm{M}y + lz)y = 0\end{aligned}\right\}$$

and eliminating dialytically we at once obtain

$$\begin{vmatrix} Ax+bz & az+cx \\ ny+mz & My+lz \end{vmatrix} = 0,$$

i.e. $(bl-am)z^2+(bM-an)yz+(Al-cm)zx+(AM-cn)xy=0,$
which is the result desired.

(4) Taking now the three original and three derived equations, we have

$$\begin{vmatrix} A & . & . & a & b & c \\ . & M & . & l & m & n \\ . & . & R & p & q & r \\ nq-mr & . & . & MR-lp & nR-mp & Mq-lr \\ . & pc-ra & . & Rc-qa & RA-qb & pA-rb \\ . & . & bl-am & bM-an & Al-cm & AM-cn \end{vmatrix}; \qquad (\alpha)$$

and this we proceed to investigate with the object of attaining further simplicity, and at the same time arriving at a form having that appearance of symmetry which may reasonably be expected from a study of the structure of the original equations.

(5) Expressing the determinant in terms of products of minors formed from the first three columns and the last three columns, we have

$$AMR\begin{vmatrix} MR-lp & nR-mp & Mq-lr \\ Rc-qa & RA-qb & pA-rb \\ bM-an & Al-cm & AM-cn \end{vmatrix} - AM(bl-am)\begin{vmatrix} p & q & r \\ MR-lp & nR-mp & Mq-lr \\ Rc-qa & RA-qb & pA-rb \end{vmatrix}$$

$$+AR(pc-ar)\begin{vmatrix} l & m & n \\ MR-lp & nR-mp & Mq-lr \\ bM-an & Al-cm & AM-cn \end{vmatrix} + A(pc-ra)(bl-am)\begin{vmatrix} l & m & n \\ p & q & r \\ MR-lp & nR-mp & Mq-lr \end{vmatrix}$$

$$-MR(nq-mr)\begin{vmatrix} a & b & c \\ Rc-qa & RA-qb & pA-rb \\ bM-an & Al-cm & AM-cn \end{vmatrix} - M(nq-mr)(bl-am)\begin{vmatrix} a & b & c \\ p & q & r \\ Rc-qa & RA-qb & pA-rb \end{vmatrix}$$

$$+R(nq-rm)(pc-ra)\begin{vmatrix} a & b & c \\ l & m & n \\ bM-an & Al-cm & AM-cn \end{vmatrix} - (nq-mr)(pc-ra)(bl-am)\begin{vmatrix} a & b & c \\ l & m & n \\ p & q & r \end{vmatrix}. \qquad (\beta)$$

(6) Again, if we multiply the determinant columnwise by

$$\begin{vmatrix} nq-mr & . & . & 1 & . & . \\ . & pc-ra & . & . & 1 & . \\ . & . & bl-am & . & . & 1 \\ -A & . & . & . & . & . \\ . & -M & . & . & . & . \\ . & . & -R & . & . & . \end{vmatrix}$$

(or, what is the same thing, multiply the fourth row by A and then subtract from it $nq-mr$ times the first row, multiply the fifth row by M and then subtract from it $pc-ra$ times the second row, multiply the sixth row by R and then subtract from it $bl-am$ times the third row) it is at once transformed into

$$\begin{vmatrix} A(MR-lp)+a(mr-nq) & n(AR-bq)+m(br-Ap) & q(AM-cn)+r(cm-Al) \\ c(MR-lp)+a(lr-Mq) & M(AR-bq)+m(ar-pc) & p(AM-cn)+r(an-Mb) \\ b(MR-lp)+a(mp-Rn) & l(AR-lq)+m(qa-Rc) & R(AM-cn)+r(am-bl) \end{vmatrix} . \qquad (\gamma)$$

(7) The form of (γ) suggests that it is a *product*, and with the suggestion in mind a little examination suffices to show that it is the quasi-product

$$\begin{vmatrix} A & n & q & mr-nq & br-Ap & cm-Al \\ c & M & p & lr-Mq & ar-pc & an-Mb \\ b & l & R & mp-Rn & qa-Rc & am-bl \end{vmatrix} . \begin{vmatrix} MR-lp & . & . & n & . & . \\ . & AR-bq & . & . & m & . \\ . & . & AM-cn & . & . & r \end{vmatrix},$$

and therefore by Binet's theorem can be expanded into

$$\begin{vmatrix} A & n & q \\ c & M & p \\ b & l & R \end{vmatrix} (MR-lp)(RA-bq)(AM-cn) + \begin{vmatrix} A & n & cm-Al \\ c & M & an-Mb \\ b & l & am-bl \end{vmatrix} (MR-lp)(AR-bq)r$$

$$- \begin{vmatrix} A & q & br-Ap \\ c & p & ar-pc \\ b & R & qa-Rc \end{vmatrix} (MR-lp)(AM-cn)m + \begin{vmatrix} A & br-Ap & cm-Al \\ c & ar-pc & an-Mb \\ b & qa-Rc & am-bl \end{vmatrix} mr(MR-lp)$$

$$+ \begin{vmatrix} n & q & mr-nq \\ M & p & lr-Mq \\ l & R & mp-Rn \end{vmatrix} (AR-bq)(AM-cn)a - \begin{vmatrix} n & mr-nq & cm-Al \\ M & lr-Mq & an-Mb \\ l & mp-Rn & am-bl \end{vmatrix} ar(AR-bq)$$

$$+ \begin{vmatrix} q & mr-nq & br-Ap \\ p & lr-Mq & ar-pc \\ R & mp-Rn & qa-Rc \end{vmatrix} am(AM-cn) + \begin{vmatrix} mr-nq & br-Ap & cm-Al \\ lr-Mq & ar-pc & an-Mb \\ mp-Rn & qa-Rc & am-bl \end{vmatrix} amr. \qquad (\beta')$$

(8) We have thus got two different but closely resembling expansions, (β) and (β') for our determinant, and when we come to compare the two carefully we discover the fact that the one is obtainable from the other by interchanging

a and A,	b and c,
m and M,	n and l,
r and R,	p and q,

—that is to say, the determinant is a function which is symmetrical with respect to the interchange

$$\begin{pmatrix} a & m & r & b & n & p \\ \updownarrow & & & & & \\ \mathrm{A} & \mathrm{M} & \mathrm{R} & c & l & q \end{pmatrix}.$$

(9) Of course an immediate deduction from this is, that if the said interchange be made in the original set of equations, the eliminant will not be altered; in other words, that the eliminant of the set

$$\left.\begin{array}{l} \mathrm{A}x^2 + ayz + bzx + cxy = 0 \\ \mathrm{M}y^2 + lyz + mzx + nxy = 0 \\ \mathrm{R}z^2 + pyz + qza + rxy = 0 \end{array}\right\}$$

is the same as the eliminant of the set

$$\left.\begin{array}{l} ax^2 + \mathrm{A}yz + czx + bxy = 0 \\ my^2 + nyz + \mathrm{M}zx + lxy = 0 \\ rz^2 + qyz + pzx + \mathrm{R}xy = 0 \end{array}\right\}.$$

This is readily established from a consideration merely of the two sets of equations. For, multiplying both sides of the original equations by yz, zx, xy respectively, we have

$$\left.\begin{array}{l} a(yz)^2 + \mathrm{A}(zx)(xy) + c(xy)(yz) + b(yz)(zx) = 0 \\ m(zx)^2 + n(zx)(xy) + \mathrm{M}(xy)(yz) + l(yz)(zx) = 0 \\ r(xy)^2 + q(zx)(xy) + \mathrm{P}(xy)(yz) + \mathrm{R}(yz)(zx) = 0 \end{array}\right\}$$

and the original problem of elimination is thus changed into another perfectly similar problem in which the variables are yz, zx, xy instead of x, y, z, and in which a takes the place of A, and A of a, etc. Or, substituting $\frac{1}{x}$, $\frac{1}{y}$, $\frac{1}{z}$ for x, y, z in the original set of equations, we obtain a set with necessarily the same eliminant, viz.,

$$\left.\begin{array}{l} \dfrac{\mathrm{A}}{x^2} + \dfrac{a}{yz} + \dfrac{b}{zx} + \dfrac{c}{xy} = 0 \\[2ex] \dfrac{\mathrm{M}}{y^2} + \dfrac{l}{yz} + \dfrac{m}{zx} + \dfrac{n}{xy} = 0 \\[2ex] \dfrac{\mathrm{R}}{z^2} + \dfrac{p}{yz} + \dfrac{q}{zx} + \dfrac{r}{xy} = 0 \end{array}\right\},$$

and this set when cleared of fractions is the second set given above.

(10) The fact that the interchange

$$\begin{pmatrix} a & m & r & b & n & p \\ \updownarrow & & & & & \\ A & M & R & c & b & q \end{pmatrix}$$

leaves the eliminant unaltered, gives us at once an alternative form for (α), viz.,

$$\begin{vmatrix} a & . & . & A & c & b \\ . & m & . & n & M & l \\ . & . & r & q & p & R \\ lp-MR & . & . & mr-nq & rl-Mq & mp-Rn \\ . & qb-RA & . & rb-Ap & ra-pc & aq-Rc \\ . & . & cn-AM & mc-Al & an-Mb & am-bl \end{vmatrix} \quad (\alpha')$$

(α) and (α') being related to each other as (β) and (β') are. This does not occur, however, in the case of (γ), because the like substitutions made there merely change rows of the determinant into columns. The form (γ) thus has an advantage over (α) and (β) in that it makes evident the symmetry which is known to exist.

To prove that (α) and (α') are identical is a good problem in the transformation of determinants.

(11) The symmetry of the eliminant, with respect to the said interchange, can also be utilised in finding the final expansion of the eliminant; because, if any one term of the expansion be got, another can at once be written down by substitution, unless, of course, the term happens to be invariant to the substitution.

The existence of the cycles may be made use of for the same purpose, as any term not symmetrical with respect to the cyclical substitutions will in this way give rise to two others.

Ordinarily, therefore, one term when found suffices to determine five others, so that the terms may be arranged in sets of six. Thus, if it be ascertained in any way that

$$bnp.clq.A.p.l$$

is a term of the expansion, we at once conclude that it is only one of three, viz.,

$$bnp.clq.A.p.l\,, \qquad bnp.clq.M.b.q\,, \qquad bnp.clq.R.n.c$$

and that corresponding to these three there are other three, viz.,

$$clq.bnp.a.q.n\,, \qquad clq.bnp.m.c.p\,, \qquad clq.bnp.r.l.b\,.$$

The sum of the first three we may denote by

$$bnp.clq.\overset{\circ}{\Sigma}\mathrm{A}.p.l\,,$$

of the second three by

$$clq.bnp.\overset{\circ}{\Sigma}a.q.n\,,$$

and of the whole six by

$$bnp.clq.\Sigma\overset{\circ}{\Sigma}\mathrm{A}.p.l$$

if we agree to use $\overset{\circ}{\Sigma}$ in connection with the cyclical substitutions of § 2, and Σ in connection with the interchange of § 8.

(12) For the purpose of calculating the final expansion of (γ) it is greatly advantageous to throw it into the form

$$\begin{vmatrix} \sigma-\rho_1 & \alpha_1 & \beta_1 \\ \beta_2 & \sigma-\rho_2 & \alpha_2 \\ \alpha_3 & \beta_3 & \sigma-\rho_2 \end{vmatrix},$$

which is authorised at once by the agreement that

$$\sigma = \mathrm{AMR} + amr$$

$$\begin{cases} \rho_1 = \mathrm{A}.p.l + a.n.q \\ \rho_2 = \mathrm{M}.b.q + m.p.c \\ \rho_3 = \mathrm{R}.n.c + r.b.l \end{cases}$$

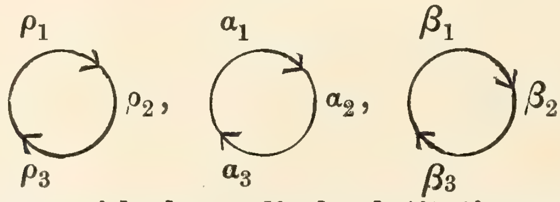
$$\begin{cases} \alpha_1 = \mathrm{RA}.n + mr.b - \mathrm{A}.m.p - bn.q \\ \alpha_2 = \mathrm{AM}.p + ra.n - \mathrm{M}.r.b - np.c \\ \alpha_3 = \mathrm{MR}.b + am.p - \mathrm{R}.a.n - pb.l \end{cases}$$

$$\begin{cases} \beta_1 = ra.l + \mathrm{MR}.c - a.\mathrm{M}.q - cl.p \\ \beta_2 = am.q + \mathrm{RA}.l - m.\mathrm{R}.c - lq.b \\ \beta_3 = mr.c + \mathrm{AM}.q - r.\mathrm{A}.l - qc.n \end{cases}$$

where it is observed

(1) that σ_1, ρ_1, ρ_2, ρ_3 are symmetrical with respect to the interchange;

(2) that α_1, α_2, α_3 are the counterparts of β_1, β_2, β_3 with respect to the interchange;

(3) that 

are cycles coexistent with the cyclical substitutions of § 2.

These matters being premised, the expansion of the determinant is seen to be

$$(\sigma-\rho_1)(\sigma-\rho_2)(\sigma-\rho_3)+\beta_1\beta_2\beta_3+\alpha_1\alpha_2\alpha_3-\mathring{\Sigma}\alpha_1\beta_2(\sigma-\rho_3)$$

or

$$\sigma^3-\sigma^2\mathring{\Sigma}\rho_1+\sigma.\mathring{\Sigma}\rho_1\rho_2-\rho_1\rho_2\rho_3+\beta_1\beta_2\beta_3+\alpha_1\alpha_2\alpha_3-\mathring{\Sigma}\alpha_1\beta_2(\sigma-\rho_3).$$

But the coefficient of σ^2 is clearly

$$-\Sigma\mathring{\Sigma}\text{A}.p.l\,;$$

and the coefficient of σ is

$$\mathring{\Sigma}\rho_1\rho_2-\mathring{\Sigma}\alpha_1\beta_2$$

which, after actual multiplication, is found to be

$$\begin{aligned}&\Sigma\mathring{\Sigma}\text{AM}.pb.lq.\ +\Sigma\mathring{\Sigma}\text{A}.m.p^2.cl\\ &-3\mathring{\Sigma}\text{AM}.am.p.q-\Sigma(\text{AMR}\mathring{\Sigma}\text{A}.p.l)+\Sigma(2amr\mathring{\Sigma}\text{A}.p.l)\\ &+\Sigma\mathring{\Sigma}2\text{AM}.pb.lq-\Sigma\mathring{\Sigma}\text{A}.m.p^2.cl-3bnp.clq.\end{aligned}$$

Writing the former term as a term in σ, viz.

$$\begin{aligned}&\sigma(\text{AMR}+amr)(-\Sigma\mathring{\Sigma}\text{A}.p.l)\\ \text{or}\quad&\sigma(-\text{AMR}\mathring{\Sigma}\text{A}.p.l-\text{AMR}\mathring{\Sigma}a.q.n-amr\mathring{\Sigma}\text{A}.p.l-amr\mathring{\Sigma}a.q.n)\\ \text{or}\quad&\sigma\{-\Sigma(\text{AMR}.\mathring{\Sigma}\text{A}.p.l)-\Sigma(amr\mathring{\Sigma}\text{A}.p.l)\},\end{aligned}$$

we see that the terms in σ^2 and σ readily combine, with the result

$$\sigma\left\{\begin{array}{l}3\Sigma\mathring{\Sigma}\text{AM}.pb.lq+\Sigma\mathring{\Sigma}\text{A}.m.p^2.cl-3\mathring{\Sigma}\text{AM}.am.p.q-\Sigma(2\text{AMR}\mathring{\Sigma}\text{A}.p.l)\\ +\Sigma(amr.\mathring{\Sigma}\text{A}.p.l)-\Sigma\mathring{\Sigma}\text{A}.m.p^2.cl-3bnp.clq.\end{array}\right\}$$

The terms independent of σ are

$$\begin{aligned}&-\rho_1\rho_2\rho_3+\alpha_1\alpha_2\alpha_3+\beta_1\beta_2\beta_3-\dot{\Sigma}\alpha_1\beta_2\rho_3\\ \text{or}\quad&-\rho_1\rho_2\rho_3+\Sigma\alpha_1\alpha_2\alpha_3-\mathring{\Sigma}\alpha_1\beta_2\rho_3\,,\end{aligned}$$

and it is found that

$$-\rho_1\rho_2\rho_3=-\sigma.bnp.clq-\Sigma\mathring{\Sigma}\text{AM}.r.pb^2.l^2q\,,$$

that

$$\begin{aligned}\Sigma\alpha_1\alpha_2\alpha_3=&(bnp+clq)(\text{A}^2\text{M}^2\text{R}^2+a^2m^2r^2-4\text{AMR}.amr)\\ &+(bnp+clq)\Sigma\mathring{\Sigma}\text{A}.p.l(amr-\text{AMR})\\ &+(bnp+clq)\Sigma\mathring{\Sigma}\text{AM}.pb.lq\\ &-(bnp+clq)bnp.clq\\ &-(bnp+clq)\mathring{\Sigma}\text{AM}.am.p.q\\ &+\Sigma(2\text{AMR}-amr)\mathring{\Sigma}\text{A}.am.np^2\\ &+\Sigma(2\text{AMR}-amr)\mathring{\Sigma}\text{A}.ra.l^2q\\ &-\Sigma bnp\mathring{\Sigma}\text{A}.m.p^2.cl\\ &-\Sigma\mathring{\Sigma}\text{A}^2\text{M}.am^2.p^3+\Sigma\mathring{\Sigma}\text{AM}^2.a.b.lq^3+\Sigma\mathring{\Sigma}\text{A}^2\text{M}.m.p^3b.l-2\Sigma\dot{\Sigma}\text{AM}.mr.p^2b^2.l\,;\end{aligned}$$

and that

$$\begin{aligned}\mathring{\Sigma}\alpha_1\beta_2\rho_3 = {} & bnp.clq\Sigma\mathring{\Sigma}\mathrm{A}.p.l \\ & - \Sigma amr\mathring{\Sigma}\mathrm{AM}.pb.lq + \Sigma\mathrm{AMR}\mathring{\Sigma}\mathrm{A}^2.p^2.l^2 - \Sigma bnp\mathring{\Sigma}\mathrm{AM}.a.lq^2 \\ & - \Sigma bnp\mathring{\Sigma}\mathrm{A}.ra.l^2q - \sigma(\Sigma\mathring{\Sigma}\mathrm{A}.m.p^2.cl - \mathring{\Sigma}\mathrm{A}.a.np.lq) \\ & + \Sigma\mathring{\Sigma}\mathrm{A}^2\mathrm{M}.am.p^2.lq + \Sigma\mathring{\Sigma}\mathrm{AM}^2.am.pb.q^2 - \Sigma\mathring{\Sigma}\mathrm{AM}^2.pb^2.lq^2 \\ & + 2\Sigma\mathring{\Sigma}\mathrm{AM}.r.pb^2.l^2q - \Sigma\mathring{\Sigma}\mathrm{A}^2\mathrm{M}.p^2b.l^2q.\end{aligned}$$

On account of the four terms in σ which here make their appearance the full coefficient of σ becomes

$$\begin{aligned} & - 4bnp.clq \\ & + \Sigma(amr - 2\mathrm{AMR})\mathring{\Sigma}\mathrm{A}.p.l \\ & - 3\mathring{\Sigma}\mathrm{AM}.am.p.q + \mathring{\Sigma}\mathrm{A}.a.np.lq \\ & + 3\Sigma\mathring{\Sigma}\mathrm{AM}.pb.lq - \Sigma\mathring{\Sigma}\mathrm{A}.m.p^2.cl. \end{aligned} \qquad (\mathrm{D})$$

The remaining terms, independent of σ, may be classified in four groups, viz. (1) those without the sign $\mathring{\Sigma}$

$$(bnp + clq)(\mathrm{A}^2\mathrm{M}^2\mathrm{R}^2 + a^2m^2r^2 - 4\mathrm{AMR}.amr - bnp.clq)\;; \qquad (\mathrm{E}_1)$$

(2) those having an expression of the 3rd degree following $\mathring{\Sigma}$

$$bnp.clq\Sigma\mathring{\Sigma}\mathrm{A}.p.l + (bnp + clq)\Sigma(amr - \mathrm{AMR})\mathring{\Sigma}\mathrm{A}.p.l\;; \qquad (\mathrm{E}_2)$$

(3) those having an expression of the 6th degree following $\mathring{\Sigma}$

$$\begin{aligned} & - (bnp + clq)\mathring{\Sigma}\mathrm{AM}.am.p.q + (bnp + clq)\Sigma\mathring{\Sigma}\mathrm{AM}.pb.lq \\ & - \Sigma amr\mathring{\Sigma}\mathrm{AM}.pb.lq + \Sigma\mathrm{AMR}\mathring{\Sigma}\mathrm{A}^2.p^2.l^2 \\ & + \Sigma(2\mathrm{AMR} - amr - clq)\mathring{\Sigma}\mathrm{A}.am.np^2 \\ & + \Sigma(2\mathrm{AMR} - amr - bnp)\mathring{\Sigma}\mathrm{A}.ra.l^2q \\ & - \Sigma bnp\mathring{\Sigma}\mathrm{A}.m.p^2.cl\;; \end{aligned} \qquad (\mathrm{E}_3)$$

(4) those having an expression of the 9th degree following $\mathring{\Sigma}$

$$\begin{aligned} & - \Sigma\mathring{\Sigma}\mathrm{A}^2\mathrm{M}.am^2.p^3 - \Sigma\mathring{\Sigma}\mathrm{AM}^2.pb^2.lq^2 - \Sigma\mathring{\Sigma}\mathrm{A}^2\mathrm{M}.p^2b.l^2q \\ & + \Sigma\mathring{\Sigma}\mathrm{A}^2\mathrm{M}.m.p^3b.l + \Sigma\mathring{\Sigma}\mathrm{AM}^2.a.b.lq^3 \\ & + \Sigma\mathring{\Sigma}\mathrm{A}^2\mathrm{M}.am.p^2.lq + \Sigma\mathring{\Sigma}\mathrm{AM}^2.am.pb.q^2 \\ & + \Sigma\mathring{\Sigma}\mathrm{AM}.r.pb^2.l^2q \\ & - 2\Sigma\mathring{\Sigma}\mathrm{AM}.mr.p^2b^2.l. \end{aligned} \qquad (\mathrm{E}_4)$$

The full expansion of the eliminant is thus

$$\sigma^3 + \sigma\mathrm{D} + \mathrm{E}_1 + \mathrm{E}_2 + \mathrm{E}_3 + \mathrm{E}_4.$$

(13) Another mode of arranging the expansion is suggested by the mode just employed for arranging that part of it which in the preceding is independent of σ. Doing this we have

(1) all the terms free of $\mathring{\Sigma}$, viz.

$$\begin{aligned}(\text{AMR}+amr)^3+(\text{A}^2\text{M}^2\text{R}^2+a^2m^2r^2-4\,\text{AMR}.amr)(bnp+clq)\\ -4(\text{AMR}+amr)bnp.clq\\ -bnp.clq(bnp+clq) \qquad (\text{F}_1)\end{aligned}$$

(2) all the terms having an expression of the 3rd degree following $\mathring{\Sigma}$, viz.

$$\begin{aligned}(\text{AMR}+amr)\Sigma(amr-2\text{AMR})\mathring{\Sigma}\text{A}.p.l+(bnp+clq)\Sigma(amr-\text{AMR})\mathring{\Sigma}\text{A}.p.l\\ +bnp.clq\Sigma\mathring{\Sigma}\text{A}.p.l \qquad (\text{F}_2)\end{aligned}$$

(3) all the terms having an expression of the 6th degree following $\mathring{\Sigma}$, viz.

$$\begin{aligned}&(\text{AMR}+amr)(\mathring{\Sigma}\text{A}.a.np.lq-\Sigma\mathring{\Sigma}\text{A}.m.p^2.cl)\\ &+(bnp+clq+3\text{AMR}+3amr)(\Sigma\mathring{\Sigma}\text{AM}.pb.lq-\mathring{\Sigma}\text{AM}.am.p.q)\\ &-\Sigma amr\mathring{\Sigma}\text{AM}.pb.lq+\Sigma\text{AMR}\mathring{\Sigma}\text{A}^2.p^2.l^2\\ &+\Sigma(2\text{AMR}-amr-clq)\mathring{\Sigma}\text{A}.am.np^2\\ &+\Sigma(2\text{AMR}-amr-bnp)\mathring{\Sigma}\text{A}.ra.l^2q\\ &-\Sigma bnp\mathring{\Sigma}\text{A}.m.p^2.cl \qquad (\text{F}_3)\end{aligned}$$

(4) all the terms having an expression of the 9th degree following $\mathring{\Sigma}$.

The full expansion is thus

$$\text{F}_1+\text{F}_2+\text{F}_3+\text{E}_4.$$

(14) There is, however, another form of the eliminant which is of far greater interest than any of the preceding. It not only bears on its face the peculiarities of structure which we know it ought to possess at least inherently, but it is also—and mainly by reason of this—much better suited for the calculation of the final expansion.

It is got by obtaining seven equations in the compound variables

$$x^2y,\ y^2z,\ z^2x,\ yz^2,\ zx^2,\ xy^2,\ xyz$$

and eliminating dialytically. The first six equations are readily

obtained from the original three by multiplying by y, z, x respectively, and then by z, x, y respectively. The seventh is got by writing the original equations in the form

$$\left.\begin{aligned}(\mathrm{A}x+cy)x &= -(bx+ay)z\\ (\mathrm{M}y+lz)y &= -(ny+mz)x\\ (\mathrm{R}z+qx)z &= -(pz+rx)y\end{aligned}\right\}$$

and multiplying, the immediate result clearly being

$$(\mathrm{A}x+cy)(\mathrm{M}y+lz)(\mathrm{R}z+qx)+(bx+ay)(ny+mz)(pz+rx)=0\,,$$

whence we have

$$(\mathrm{AM}q+bnr)x^2y+(\mathrm{MR}c+anp)y^2z+\;.\;.\;.\;.\;=0\,.$$

The new form is thus seen to be

$$\left|\begin{array}{ccccccc}\mathrm{A} & a & . & . & . & c & b\\ . & \mathrm{M} & m & l & . & . & n\\ r & . & \mathrm{R} & . & q & . & p\\ . & . & b & a & \mathrm{A} & . & c\\ n & . & . & . & m & \mathrm{M} & l\\ . & p & . & \mathrm{R} & . & r & q\\ \mathrm{AM}q+bnr & \mathrm{MR}c+anp & \mathrm{AR}l+bmp & \mathrm{R}lc+amp & \mathrm{A}ql+mrb & \mathrm{M}cq+anr & \sigma+\sigma'\end{array}\right| \qquad (\delta)$$

where, for shortness' sake, $\sigma+\sigma'$ is written for $\mathrm{AMR}+amr+bnp+clq$.

(15) To show that this is symmetrical with respect to the interchange of § 8, we have only got to interchange three of the columns with other three, viz., 1, 2, 3 with 4, 5, 6 respectively, and then three of the rows with other three, viz., 1, 2, 3 with 4, 5, 6 respectively, when it is found that the interchange of § 8 has been effected.

The symmetry with respect to the cyclical substitutions is equally easily made manifest. Taking, for example, the substitution

$$\begin{pmatrix}\mathrm{A} & \mathrm{M} & \mathrm{R} & a & m & r & b & n & p & c & l & q\\ \downarrow & & & & & & & & & & & \\ \mathrm{M} & \mathrm{R} & \mathrm{A} & m & r & a & n & p & b & l & q & c\end{pmatrix}$$

we find that the effect of it upon the determinant is exactly the same as the effect of passing the 1st column over into the 3rd place,

the 4th column into the 6th place, and thereafter the 1st row into the 3rd place and the 4th row into the 6th place.

(16) A consequence of this is that if the determinant be expressed in terms of the elements of the last row and their complementary minors, only two of the seven resulting expressions require to be fully expanded in order to obtain the final development. The first of the two is

$$(\sigma + \sigma') \times \text{ its complementary minor},$$

and the second is any one of the remaining six, say

$$(\mathrm{AM}q + bnr) \times \text{ its complementary minor.}$$

The former is invariant to all the substitutions; the latter by means of the substitutions gives rise to the remaining five similar expressions. In other words, the eliminant may be written

$$(\mathrm{AMR} + amr + bnp + clq) \begin{vmatrix} \mathrm{A} & a & . & . & . & c \\ . & \mathrm{M} & m & l & . & . \\ r & . & \mathrm{R} & . & q & . \\ . & . & b & a & \mathrm{A} & . \\ n & . & . & . & m & \mathrm{M} \\ . & p & . & \mathrm{R} & . & r \end{vmatrix}$$

$$+ \Sigma\mathring{\Sigma}(\mathrm{AM}q + bnr) \begin{vmatrix} a & . & . & . & c & b \\ \mathrm{M} & m & l & . & . & n \\ . & \mathrm{R} & . & q & . & p \\ . & b & a & \mathrm{A} & . & c \\ . & . & . & m & \mathrm{M} & l \\ p & . & \mathrm{R} & . & r & q \end{vmatrix},$$

where, it may be noted, the first determinant equals

$$(\mathrm{AMR} + amr)^2 - \Sigma\mathrm{AMR}\dot{\Sigma}\mathrm{A}.p.l + \Sigma\mathring{\Sigma}\mathrm{AM}.pb.lq - \mathring{\Sigma}\mathrm{AM}.am.pq - bnp.clq.$$

(17) Strange to say there is another form of the eliminant, which has all the properties of that of § 14, and differs from it only in the last row. This is due to the fact that from the original three equations there is another way of obtaining an equation in

$$x^2y,\ y^2z,\ z^2x,\ yz^2,\ zx^2,\ xy^2,\ xyz.$$

Instead of writing the first equation in the form

$$(\mathrm{A}x+cy)x = -(bx+ay)z\,,$$

we make the simple change

$$\left.\begin{aligned} &(\mathrm{A}x+bz)x = -(cx+az)y, \\ \text{and similarly,}\quad &(\mathrm{M}y+nx)y = -(ly+mx)z, \\ \text{and}\quad &(\mathrm{R}z+py)z = -(qz+ry)x\,; \end{aligned}\right\}$$

whence by multiplication there results

$$(\mathrm{A}x+bz)(\mathrm{M}y+nx)(\mathrm{R}z+py)+(cx+az)(ly+mx)(qz+ry)=0\,,$$

or

$$\begin{aligned} &(\mathrm{A}np+cmr)x^2y+(\mathrm{M}pb+lra)y^2z+(\mathrm{R}bn+qam)z^2x \\ &+(\mathrm{MR}b+alq)yz^2+(\mathrm{RA}n+mqc)zx^2+(\mathrm{AM}p+rcl)xy^2 \\ &\qquad+(\mathrm{AMR}+amr+bnp+clq)xyz \qquad =0\,. \end{aligned}$$

Instead, therefore, of the 7th row of the determinant of § 14 we may substitute the row

$$\mathrm{A}np+cmr \quad \mathrm{M}pb+lra \quad \mathrm{R}bn+qam \quad \mathrm{MR}b+alq \quad \mathrm{RA}n+mqc \quad \mathrm{AM}p+rcl \qquad \sigma+\sigma'\,;$$

and since the last element of the new 7th row is not itself new, it follows that

$$\mathring{\Sigma}\Sigma(\mathrm{AM}q+bnr)\begin{vmatrix} a & . & . & . & c & b \\ \mathrm{M} & m & l & . & . & n \\ . & \mathrm{R} & . & q & . & p \\ . & b & a & \mathrm{A} & . & c \\ . & . & . & m & \mathrm{M} & l \\ p & . & \mathrm{R} & . & r & q \end{vmatrix} = \mathring{\Sigma}\Sigma(\mathrm{A}np+cmr)\begin{vmatrix} a & . & . & . & c & b \\ \mathrm{M} & m & l & . & . & n \\ . & \mathrm{R} & . & q & . & p \\ . & b & a & \mathrm{A} & . & c \\ . & . & . & m & \mathrm{M} & l \\ p & . & \mathrm{R} & . & r & q \end{vmatrix},$$

or that

$$\begin{vmatrix} \mathrm{A} & a & . & . & . & c & b \\ . & \mathrm{M} & m & l & . & . & n \\ r & . & \mathrm{R} & . & q & . & p \\ . & . & b & a & \mathrm{A} & . & c \\ n & . & . & . & m & \mathrm{M} & l \\ . & p & . & \mathrm{R} & . & r & q \\ (7,1) & (7,2) & (7,3) & (7,4) & (7,5) & (7,6) & . \end{vmatrix} = 0,$$

$$\begin{aligned}
\text{where } (7,1) &= \mathrm{A}(\mathrm{M}q - np) + r(bn - cm), \\
\text{,, } (7,2) &= \mathrm{M}(\mathrm{R}c - pb) + a(np - lr), \\
\text{,, } (7,3) &= \mathrm{R}(\mathrm{A}l - bn) + m(pb - qa), \\
\text{,, } (7,4) &= a(mp - lq) + \mathrm{R}(cl - b\mathrm{M}), \\
\text{,, } (7,5) &= m(rb - qc) + \mathrm{A}(lq - n\mathrm{R}), \\
\text{,, } (7,6) &= r(an - cl) + \mathrm{M}(qc - p\mathrm{A}).
\end{aligned}$$

This can be directly established by increasing each element of the 7th row by

$lr - \mathrm{M}q$	times the corresponding element of the 1st row,			
$qa - \mathrm{R}c$	,,	,,	,,	2nd ,,
$cm - \mathrm{A}l$	,,	,,	,,	3rd ,,
$n\mathrm{R} - mp$	,,	,,	,,	4th ,,
$p\mathrm{A} - rb$	,,	,,	,,	5th ,,
and $b\mathrm{M} - an$	,,	,,	,,	6th ,,

when it will be found that the result is 0 in every case.

(18) The determinant (δ) of § 14 and the similar form of § 17 may be modified so as to give a form of the eliminant possessing all the properties of (δ) and at the same time having an additional simplicity of form. This modification is effected in the case of (δ) by diminishing each element of the last row

by $\mathrm{M}q$	times the corresponding element of the 1st row			
,, $\mathrm{R}c$	,,	,,	,,	2nd ,,
,, $\mathrm{A}l$	,,	,,	,,	3rd ,,
,, mp	,,	,,	,,	4th ,,
,, rb	,,	,,	,,	5th ,,
,, an	,,	,,	,,	6th ,,

the result being

$$\begin{vmatrix}
\mathrm{A} & a & \cdot & \cdot & \cdot & c & b \\
\cdot & \mathrm{M} & m & l & \cdot & \cdot & n \\
r & \cdot & \mathrm{R} & \cdot & q & \cdot & p \\
\cdot & \cdot & b & a & \mathrm{A} & \cdot & c \\
n & \cdot & \cdot & \cdot & m & \mathrm{M} & l \\
\cdot & p & \cdot & \mathrm{R} & \cdot & r & q \\
-\mathrm{A}lr & -\mathrm{M}qa & -\mathrm{R}cm & -an\mathrm{R} & -mp\mathrm{A} & -rb\mathrm{M} & \Delta+\Delta'-\sigma'
\end{vmatrix} \qquad (\epsilon)$$

where Δ and Δ' are the third-order determinants of § 1, and consequently $\Delta + \Delta' - \sigma' = \sigma + \sigma' - \Sigma\mathring{\Sigma} A.p.l.$

Expressing this determinant in terms of the elements of the last row and their complementary minors, we have a form similar to that of § 16, viz.

$$(\Delta + \Delta' - \sigma') \begin{vmatrix} A & a & . & . & . & c \\ . & M & m & & . & . \\ r & . & R & . & q & . \\ . & . & b & a & A & . \\ n & . & . & . & m & M \\ . & p & . & R & . & r \end{vmatrix} - \Sigma\mathring{\Sigma} Alr \begin{vmatrix} a & . & . & . & c & b \\ M & m & l & . & . & n \\ . & R & . & q & . & p \\ . & b & a & A & . & c \\ . & . & . & m & M & l \\ p & . & R & . & r & q \end{vmatrix} .$$

(19) There can be little doubt that this is the best form yet found. On putting either $A = M = R = 0$ or $a = m = r = 0$, the terms prefaced by $\Sigma\mathring{\Sigma}$ all vanish, the coefficient $\Delta + \Delta' - \sigma'$ becomes either Δ or Δ', and the first determinant becomes the product of the coaxial minors of Δ or Δ'.

On the so-called "Hypoiodite of Magnesium." By James Walker, D.Sc., Ph.D., and Sydney A. Kay, B.Sc., University College, Dundee.

(Read December 7, 1896.)

The following investigation was undertaken in connection with a series of experiments on absorption at present being carried out in this laboratory.

In Gmelin's *Handbook* (Cavendish Edition, vol. 3, p. 240) it is stated, on Gay-Lussac's authority, that magnesia, when shaken up with a solution of iodine, absorbs the latter and assumes a reddish-brown colour. The production of this brown "hypoiodite of magnesium" is accompanied by the formation of small quantities of magnesium iodide and magnesium iodate, which pass into solution. When the brown compound is boiled with water, magnesium iodide and magnesia are formed, and when it is heated by itself, iodine is given off and magnesia remains.

The same compound is produced when magnesia is precipitated in presence of iodine, and its characteristic appearance makes it an excellent means of identifying a magnesium salt. The test is described as follows in the *Notes on Reactions of Salts*, used in the Practical Chemistry Class in the University of Edinburgh. "When a solution of iodine in potassium iodide is added to a magnesium salt, and a few drops of caustic potash are added to the mixture, a reddish-brown precipitate is produced; excess of caustic potash decolorises the precipitate."

The state of the magnesia has apparently little effect on the production of the brown substance. It is formed indifferently with freshly precipitated magnesia, magnesia dried at the ordinary temperature, magnesia dried at 100°, and magnesia ignited over the blow-pipe. It is true that the freshly precipitated compound is more active than any of the other forms, the difference being possibly due to the loose flocculent nature of the precipitate, or perhaps to its being the hydroxide and not the oxide. The ignited oxide is least readily coloured by the iodine.

The production of the brown substance seems likewise to be independent of the state of the iodine. Iodine in solution in water, in aqueous potassium iodide, in alcohol, in chloroform, or in carbon disulphide at once unites with magnesia, staining it deep brown. The vapour of iodine, too, is equally effective. If two watch-glasses, containing iodine and magnesia respectively, are placed beneath an inverted glass dish, it will be seen that the magnesia rapidly acquires a coffee-brown colour, especially if the laboratory temperature is moderately high. The colour is not superficial, but penetrates uniformly into the interior of the powder. Some other oxides (for example, calcium oxide), when exposed to the vapour of iodine under the same conditions, acquire a superficial brownish tint, but this is by no means uniform even on the surface, and differs greatly in appearance from the brown of the magnesia. Zinc oxide is stained slowly to a dirty pink, the colour on the surface differing somewhat from the colour in the interior. Pure white salts, such as precipitated calcium carbonate, are quite unaffected.

It is perhaps worthy of note that the coloration at once takes place when iodine and magnesia are simultaneously produced in the same solution. If to a solution of iodine in aqueous potassium iodide potash is cautiously added until the iodine has completely disappeared, the addition of a small quantity of magnesium sulphate will immediately produce a chocolate-brown precipitate in the pale yellow hypoiodite solution. Expressed in terms of the electrolytic dissociation theory the action which takes place is as follows. Hypoiodite ions can only exist in presence of iodide ions when hydroxyl ions are also present in certain proportions; if the hydroxyl ions are removed, the other ions react with water to reproduce hydroxyl ions, iodine being at the same time liberated. There is, in fact, a balanced action expressible by the equation,

$$I_2 + 2OH' \rightleftarrows I' + OI' + H_2O,$$

and if this balance be disturbed by the removal of hydroxyl ions, more hydroxyl ions will be produced, and their production must be accompanied by the liberation of iodine. Now the addition of a magnesium salt removes hydroxyl ions from the solution in the hydroxide which is precipitated, and this hydroxide is at once

coloured by the iodine set free at the same time. That this view of the process is essentially correct may be seen from the following experiment. A solution of hypoiodous acid, made by agitating a pure aqueous solution of iodine with mercuric oxide and filtering, is rapidly neutralised with potash and a magnesium salt added. The precipitate obtained is white. Addition of a few drops of potassium iodide solution will now stain the precipitate brown, showing that the presence of iodide ions is essential to the action. The precipitate is also brown when the hypoiodite and iodide solutions are first mixed and then treated with magnesium sulphate.

When the precipitate obtained from an aqueous solution is dried either at the ordinary temperature or at 100°, it is very dark in appearance, being almost black, whilst the substance prepared by colouring the magnesia with iodine in chloroform solution has approximately the colour of magnesia stained with iodine vapour. Regarding the stability of the dried substances prepared by the various methods, it may be said that they all give off iodine vapour at the ordinary temperature. The amount of iodine given off is undoubtedly very small, but the presence of iodine vapour may be made evident as follows. The sample to be tested is placed in a closed space alongside a small quantity of magnesia. After a little time the magnesia becomes brown, the depth of tint varying considerably, however, with different specimens. This process forms a convenient mode of testing for the presence of iodine vapour, especially where the absence of moisture is essential. The material suspected to give off iodine is placed near a small quantity of magnesia on a white tile, both substances being then covered by a watch-glass. After 5–10 minutes the magnesia may be examined, and will then reveal the presence of even minute traces of iodine. The following example of its application may be mentioned. Magnesium iodide as purchased is of a brown colour. It was suspected that this might be due to partial decomposition of the iodide with formation both of magnesia and of iodine, which could then unite to give the "hypoiodite." A little of the iodide was accordingly brought into juxtaposition with magnesia, when it at once imparted a brown colour to the latter, showing that it gave off iodine vapour. That our surmise was correct became further evident on dissolving the iodide in water. The bulk of the solid

dissolved, but minute brown flocks remained behind, and these were proved to have properties identical with those of the "hypoiodite."

When the "hypoiodite" precipitated from aqueous solution was heated at gradually increasing temperatures, the violet vapour of iodine was first observed at 130°, but even at 230° the decomposition was by no means rapid. A quantity of the substance was introduced into one limb of a glass tube bent at right angles, the tube being then exhausted and sealed off. The empty limb was kept in cold water while the charged limb was heated in an oil-bath. After prolonged heating at 230°, very little iodine had condensed in the cool part of the tube, although the violet vapour could be distinctly seen. In the vapour of boiling anthracene (about 350°) the iodine still came off very slowly, and even after many hours' heating the residue remained almost as deeply coloured as at first. A quantitative experiment resulted as follows. An amount of "hypoiodite," which when dissolved in acid reacted with 9·5 c.c. of decinormal sodium thiosulphate, after twelve hours' heating in anthracene vapour required 1·65 c.c. of the same solution to destroy the iodine liberated on dissolving the residue in hydrochloric acid. Thus 17 per cent. of the original quantity of iodine remained in the magnesia, the other portion having condensed in the cool parts of the tube. At a red heat iodine was given off rapidly and magnesia remained behind.

Vapour-stained magnesia was still deeply coloured after exposure to a current of carbonic acid for several hours.

Liquids in which iodine is soluble extract iodine from the "hypoiodite" only very slowly. Thus a quantity of "hypoiodite" corresponding to 11·4 c.c. of decinormal thiosulphate was treated in a Soxhlet apparatus for twenty-four hours with carbon tetrachloride (b.p. 75°). At the end of that time the residue was dark brown and required 6·0 c.c. of thiosulphate to destroy the iodine liberated on acidification. Accordingly, less than half the iodine had been removed by the prolonged extraction. A specimen, which had been prepared in chloroform solution with ignited magnesia, was similarly treated with chloroform in a Soxhlet extractor. After thirty hours it was still coloured, and still yielded iodine, although in very small quantity, when treated with a fresh portion of the solvent.

Water does not dissolve out free iodine from the "hypoiodite" either at the ordinary temperature or at the boiling-point. This is probably due to the slight solubility of iodine in water, enough magnesia being simultaneously dissolved to convert the iodine into iodide and iodate. The filtered aqueous solution at once gives free iodine on acidification. A small quantity of the "hypoiodite" left in water at the ordinary temperature becomes decolorised after several days, or disappears entirely. In boiling water it loses its colour in a few minutes.

An aqueous solution of potassium iodide extracts iodine from the "hypoiodite" at the ordinary temperature. If the liquid is decanted off and the precipitate treated with successive fresh portions of the iodide solution, iodine continues to be extracted in gradually diminishing quantity until the residue is colourless. In the last extracts there is no free iodine, but iodine is liberated from them on acidification.

Quantitative Experiments.

In order to ascertain if the "hypoiodite" were a real chemical compound or not it was necessary for us to perform quantitative experiments, as a study of the qualitative behaviour of the body alone could lead to no definite conclusions. We wished in particular to determine if the substance were not analogous to the so-called iodide of starch, which has recently been shown by Küster (*Liebig's Annalen*, 1894, 283, 360) to be no true chemical compound in the ordinary sense, but a substance of composition varying with the strength of the iodine solution used in its preparation.

A preliminary set of experiments was made by shaking up magnesia with decinormal solution of iodine in aqueous potassium iodide for varying times, in order to determine the approximate amount of iodine absorbed, and that converted into iodide and iodate under these conditions. After shaking for a definite time the brown solid was filtered off, drained, and washed free from adhering solution with a little water. So long as calcined magnesia was employed, no difficulty was experienced in filtering and washing the precipitate, but when the magnesia was formed in the solution, as was the case in the majority of the later experiments, the precipitate was rather unmanageable. A Gooch crucible with a fine

asbestos felt was in the end adopted as giving the most satisfactory results in the filtration. The filtrate was titrated with sodium thiosulphate solution equivalent in strength to the original solution of iodine. The quantity of thiosulphate required gave the amount of free iodine remaining in the solution. The liquid was next acidified in order to liberate the iodine contained in the solution as iodate and iodide, and again titrated. Lastly, the precipitate was dissolved in hydrochloric acid, with addition of potassium iodide to effect the solution of the liberated iodine, which was then titrated with thiosulphate.

With freshly calcined magnesia it was found that the quantity of iodine remaining in a given solution diminished as the time during which the solution was agitated increased. Thus 50 c.c. of decinormal iodine solution, on being shaken with 1 g. of magnesia, gave the following numbers—

Time of Agitation.	Titre of Filtered Solution.
15 minutes	29·4 c.c.
30 ,,	27·0 c.c.
60 ,,	20·9 c.c.

As we thought that the absorption might proceed more rapidly and uniformly if precipitated magnesia were used instead of finely divided calcined magnesia, a seminormal solution of caustic soda was prepared, and measured quantities of it were added to a slight excess of magnesium sulphate solution. The magnesia was thus precipitated in a loose flocculent state and very rapidly absorbed iodine, prolonged agitation increasing the amount absorbed, as the following numbers show. In the first set the solution was filtered immediately after the addition of the iodine; in the second set after shaking for fifteen minutes.

Iodine taken.	Iodine remaining.	Iodine absorbed.	Iodine transformed.	Error.
5	2·28	1·42	1·20	0·10
5	1·89	1·81	1·21	0·09

In this as in our other tables for aqueous solutions the numbers have been reduced so as to refer to seminormal solutions and 10 c.c. of caustic soda. It will be noted that the sum of the amounts of iodine remaining, absorbed, and transformed into iodide and iodate falls somewhat short of the amount originally taken, the difference being given in the last column. Special experiments pointed to the deficiency, which was always small, arising in the titration of the dissolved precipitate; so that its amount should probably be added to the quantity absorbed. In subsequent tables this correction has been made, so that in them the "absorbed iodine" is really the original iodine minus the total quantity found in the filtered solution before and after acidification.

From the figures given above it will be seen that the extent of the chemical action is considerable, a comparatively large proportion of the iodine being converted into iodide and iodate. In calculating the amount of iodine combined with the magnesia to form the "hypoiodite," it is therefore necessary to subtract the proportion of the original magnesia which has passed into solution on account of the chemical transformation. Thus, in the above example, of 10 equivalents of magnesia taken, 1·2 equivalents have been converted into soluble salts, so that only 8·8 equivalents remain, and these have absorbed 1·42 equivalents of iodine. Ten equivalents of magnesia have therefore absorbed 1·61 equivalents of iodine in the first set, and 2·01 equivalents in the second. In the tables which follow, this calculation has been performed, and we give the number of equivalents of iodine absorbed by 10 equivalents of magnesia, or what is the same thing, I_2 in 10MgO.

A series of experiments was made in which the quantity of iodine alone was varied, the time of shaking before filtration being in each case ten minutes.

Volume.	Iodine taken.	Iodine remaining.	I_2 in 10MgO.	Iodine transformed.
30 c.c.	2·50	0·57	1·31	0·62
,,	2·00	0·22	1·29	0·59
,,	1·50	0·11	1·02	0·42
,,	1·00	0·067	0·70	0·25

Here it is evident that both the iodine which is chemically transformed and the iodine which is absorbed, fall off with the amount of iodine originally present, but in no case is the iodine wholly destroyed as such; and although the iodine in the magnesia of the first experiment is greater in amount than the iodine originally taken in the fourth experiment, yet in the latter case some of the iodine remains unabsorbed. It would appear, therefore, from a consideration of these numbers, that there is an equilibrium between the iodine remaining in the solution and the iodine absorbed by the magnesia, in accordance with the results of the extraction experiments previously referred to.

The only difference between the following set of experiments and the foregoing is that the magnesia, instead of being precipitated before the addition of the iodine, was precipitated in presence of the iodine. It was hoped that in this way the magnesia in process of formation from magnesium sulphate and caustic soda would at once saturate itself with iodine; and also that the amount of iodine chemically transformed would be reduced. The solution was in each case filtered one minute after the addition of the caustic soda.

Volume.	Iodine taken.	Iodine remaining.	I_2 in 10MgO.	Iodine transformed.
30 c.c.	2·50	0·132	1·92	0·54
,,	2·00	0·039	1·63	0·39
,,	1·50	0·035	1·24	0·25
,,	1·00	0·025	0·80	0·19

Our expectations were to some extent fulfilled, as the amount of iodine absorbed was in each case greater than in the former series, and the amount chemically transformed in each case less. On the whole, however, the general aspect of the table remains unchanged. An experiment in which the filtration was delayed for fifteen minutes after the addition of the soda resulted as follows:—

Volume.	Iodine taken.	Iodine remaining.	I_2 in MgO.	Iodine transformed.
30 c.c.	2·5	0·077	1·84	0·69

These figures show that continued agitation increases the amount

chemically transformed at the expense of both the iodine absorbed and the iodine remaining in the solution.

One of the simplest means of determining the nature of an equilibrium is to ascertain the effect on it of mere dilution of the reacting substances, their amounts remaining unchanged. We accordingly performed several sets of experiments, in which the quantity of water alone was varied for each set. In the tables exhibiting our results we have added a column which gives the final concentration of the iodine solution, the concentration of the strongest iodine solution after the absorption being made equal to 100.

Volume.	Iodine taken.	Iodine remaining.	Final concentration.	I_2 in 10MgO.	Iodine transformed.
		First Series.			
60 c.c.	16	12·5	100	2·84	0·92
120 ,,	,,	11·56	46	2·98	2·08
200 ,,	,,	11·56	28	2·96	2·10
300 ,,	,,	10·92	17	3·27	2·68
400 ,,	,,	10·56	12	3·04	3·47
		Second Series.			
60 c.c.	8	4·47	35	2·60	1·22
120 ,,	,,	3·94	15	2·57	2·01
200 ,,	,,	3·04	7·2	2·50	3·28
300 ,,	,,	3·14	5·0	2·47	3·18
		Third Series.			
60 ,,	4	1·36	10·9	1·96	0·81
120 ,,	,,	0·90	3·6	1·92	1·44
200 ,,	,,	0·56	1·3	1·90	1·89
300 ,,	,,	0·42	0·67	1·70	2·26

It is very evident from these tables that increase in the dilution of the reacting substances greatly increases the amount of iodine chemically transformed. The effect of dilution on the amount of iodine absorbed by the magnesia is not so marked. In the first series there is an apparent increase, in the other series there is slight diminution. If the iodine in the magnesia is estimated

directly and not by difference as in the tables, the numbers for the absorbed iodine become more nearly constant in the first series, and diminish somewhat more rapidly in the others. On the whole, it may be said that for given amounts of iodine and magnesia, the effect of varying quantities of water on the absorption is very slight. If the quantity of water and of magnesia is kept constant, and the iodine is varied, there is a falling-off in the amount absorbed as the total quantity of iodine diminishes. This is apparent when we compare corresponding volumes in the different sets, and confirms the results given on the preceding page. An inspection of the numbers for all the series shows that there is no definite relationship between the final concentration of the iodine solution and the concentration of iodine in the magnesia. For example, in the last experiment of the first series, the final concentration of the aqueous solution is 12, and the amount of absorbed iodine 3·04; whilst in the first experiment of the next series, the much greater final concentration 35 corresponds to a smaller absorption, viz., 2·60. This want of correspondence between the concentrations of the iodine in the water and in the magnesia renders the results obtained in aqueous solutions useless for determining the nature of the "hypoiodite." It is, however, highly probable, even from a consideration of them alone, that the "hypoiodite" is not a real chemical compound partially decomposable by water into iodine and magnesia. If it were so, we should expect that within certain limits the concentration of iodine in the water would remain the same, while the concentration in the magnesia varied (compare Walker and Appleyard, *Jour. Chem. Soc.*, 1896, 69, 1341). The disturbing cause in these experiments is no doubt the chemical action which occurs to so great an extent simultaneously with the absorption; and in order to eliminate this source of error we made a number of experiments with iodine in chloroform solution, the magnesia employed being the ignited oxide. Here there is no water, and the formation of iodide and iodate does not take place.

The results that we obtained at first were very singular, when the substances employed were dried with ordinary precautions. The magnesia was weighed off and shaken up in stoppered cylinders with a chloroform solution of iodine, by means of a mechanical arrangement driven by a small turbine. The absorption

at first was fairly rapid, but soon became slower, iodine disappearing gradually from the solution day after day. In a very dilute solution all the iodine apparently passed into the magnesia, the chloroform being left absolutely colourless. This pointed to no true equilibrium existing under the conditions employed. Again, a series of experiments in which the ratio of iodine to magnesia was constant, the amount of chloroform alone being varied, showed the remarkable result that more iodine disappeared from the dilute than from the concentrated solutions. These anomalies were evidently due to some chemical action which destroyed the iodine, and special precautions were then adopted for the removal of moisture from the reacting substances and the vessels which contained them. It was found that consistent results were obtained only when the magnesia employed had been freshly ignited and cooled in a desiccator. The chloroform was carefully dried with calcium chloride, kept over sodium, and distilled from fresh slices of sodium immediately before use. The iodine from which the solutions were made had stood for at least several days over strong sulphuric acid in a desiccator, and the cylinders employed were dried at 120° in an air oven. As much expedition as possible had to be used in weighing off the magnesia, otherwise the experiments failed. It was found impracticable to weigh off the magnesia and then ignite it just before it was added to the iodine solution, for specimens ignited under as nearly as possible the same conditions gave very different results, so that no comparison between them was possible. To preclude any action of light the cylinders containing the solutions were wrapped in dark paper throughout the absorption. Even when the above precautions were taken to ensure the absence of appreciable quantities of moisture, considerable difficulty was experienced in obtaining trustworthy results. Preliminary experiments showed that after five days the concentration of the iodine solution no longer diminished perceptibly, and that continued agitation appeared to have very little effect on the time necessary to saturate the magnesia. The iodine in the chloroform solution was determined by shaking up a known volume with a known volume of thiosulphate solution and titrating the excess of the latter with a standard solution of iodine.

A first series of experiments with a constant quantity of magnesia, viz., 0·4 gram, and a constant volume of solution, viz., 25 c.c., resulted as follows, the numbers being in terms of decinormal iodine solution.

Iodine taken.	Iodine remaining.	Iodine absorbed.
27·0	23·60	3·40
13·5	10·85	2·65
6·75	4·60	2·15
3·37	1·85	1·52

Here, as in the case of aqueous solutions, the concentration of iodine in magnesia diminishes with the concentration of the chloroform solution, but less rapidly than the latter. The actual amount of iodine absorbed is much smaller than before, being in the first experiment 0·17 I_2 in 10MgO, compared with about 3·0 I_2 in 10MgO from a corresponding aqueous solution. The hydroxide is thus much more capable of absorbing iodine than the ignited oxide.

A set of experiments in which the ratio of magnesia to iodine was kept constant, the amount of chloroform alone being varied, was also made, with the following results :—

Magnesia.	Volume.	Iodine taken.	Iodine remaining.	Concentration.	Iodine absorbed.
0·4 g.	25 c.c.	26·82	24·56	24·56	2·26
,,	50 ,,	,,	25·26	12·63	1·56
,,	100 ,,	,,	25·70	6·42	1·12
,,	200 ,,	,,	26·80	3·35	0·02

Again the amount absorbed by the magnesia falls off as the concentration of the solution diminishes, the decrease being very marked and comparable with the decrease when the total volume remained constant and the amount of iodine was varied. The second set of experiments cannot be compared directly with the first, as it was necessary to reignite the magnesia. Other series with varying proportions were made, and although the numbers

obtained differed considerably from set to set, the general result was the same as that indicated in the tables given above. In any one set the amount of iodine absorbed by the magnesia diminished according as the final concentration of the chloroform solution diminished, whether this diminution was due to a smaller quantity of iodine, or a greater volume of chloroform being taken. The phenomena are therefore much simpler than when aqueous iodine is used, the absorption being there complicated by chemical action occurring to an equal or even greater extent.

Taking into consideration both the qualitative and the quantitative experiments, it is apparent that we are here dealing with a case of absorption analogous to the absorption of acids by silk (Walker and Appleyard, *loc. cit.*), and to the absorption of iodine by starch to form the blue "iodide of starch." The "hypoiodite" shows no behaviour truly characteristic of a chemical compound, unless the extraordinary tenacity with which the iodine clings to the magnesia be accounted such. This, however, is not uncommon in cases of absorption. It is, for instance, a matter of extreme difficulty to remove from charcoal an acid which it has absorbed; and platinum-black behaves with regard to some substances in a precisely similar way.

Küster (*loc. cit.*) found that the amount of iodine absorbed by starch varied with the concentration of the iodine solution employed, though much less rapidly, and this we have found to be the case with the absorption of iodine by magnesia. From his observations Küster concludes that the blue iodide of starch is neither a chemical compound nor a mechanical mixture of iodine and starch, but a well-defined solution of iodine in starch. In this conclusion he is, in our opinion, scarcely justified, if the word "solution" is employed in the sense of the "solid solution" of van't Hoff. His experiments show that $\sqrt[10]{K_w} : K_s$ is constant, where K_w is the concentration of the aqueous solution, and K_s the concentration in the starch. Now, were the "iodide of starch" a solid solution, a necessary deduction from these numbers would be that iodine dissolved in water must have a molecular weight ten times greater than iodine "dissolved" in starch, which is contrary to all experience. The same objection holds good against the "solid solution" theory of dyeing, and against a "solid solution"

theory of "magnesium hypoiodite." In nearly all cases of absorption from solution the amount absorbed varies as a root of the final concentration of the solution. This simply means that a very slight percentage change in the concentration of the solution always occasions a proportional percentage change in the amount absorbed, no matter what the absolute values may be. It is true that "solid solutions" form a particular case of this general rule, but it does not therefore follow that all absorptions which come under the rule are solid solutions.

The general result of this investigation is, then, that so far as our experiments extend, the absorption of iodine by magnesia is similar to other cases of absorption, though what the nature of such absorptions may be is at present undetermined.

Abstract of Paper "On Intermediary Links between Man and the Lower Animals." By Robert Munro, M.A., M.D.

(Read January 4, 1897.)

Dr Munro maintained in this paper that, by the attainment of the erect posture and the consequent conversion of his limbs into hands and feet, man became *homo sapiens*, and inaugurated a new phase of existence, by means of which the manipulative organs became correlated with the progressive development of the brain. In the evolutionary career of man two stages were therefore to be recognised. First, that during which his physical transformation had been effected, so as to adapt him to bipedal locomotion; second, that during which his mental organisation had become a new governing force in the universe. The one, being readily effected in accordance with the laws of morphological adaptation, had a short duration. The other, an extremely slow process, consisted of small increments to his knowledge, acquired by repeated experiences of reasoning from causes to effects and from means to ends. The one was merely an adjustment of physical contrivances to physical ends, comparable to that by which the bird, the bat, and the whale had converted their limbs to their special purposes. The other had to be relegated to the mystic laboratory, where thought was converted into its material equivalent in the form of increased brain substance. The transition from the semi-erect to the erect posture could not, in point of duration, be at all paralleled with the ages during which this erect being had lived on the globe. It was also probable that this transformation took place in a limited area, so that the chances of finding the intermediary links of this stage were very small. On the other hand, the probability of finding erect beings with skulls in all grades of development, from a slightly changed Simian type up to that of civilised man, was enormously greater. He regarded the erect posture as the most conspicuous line of demarcation between man and the lower animals. From this standpoint the Java skeleton would come under the category of human; but, if this line of

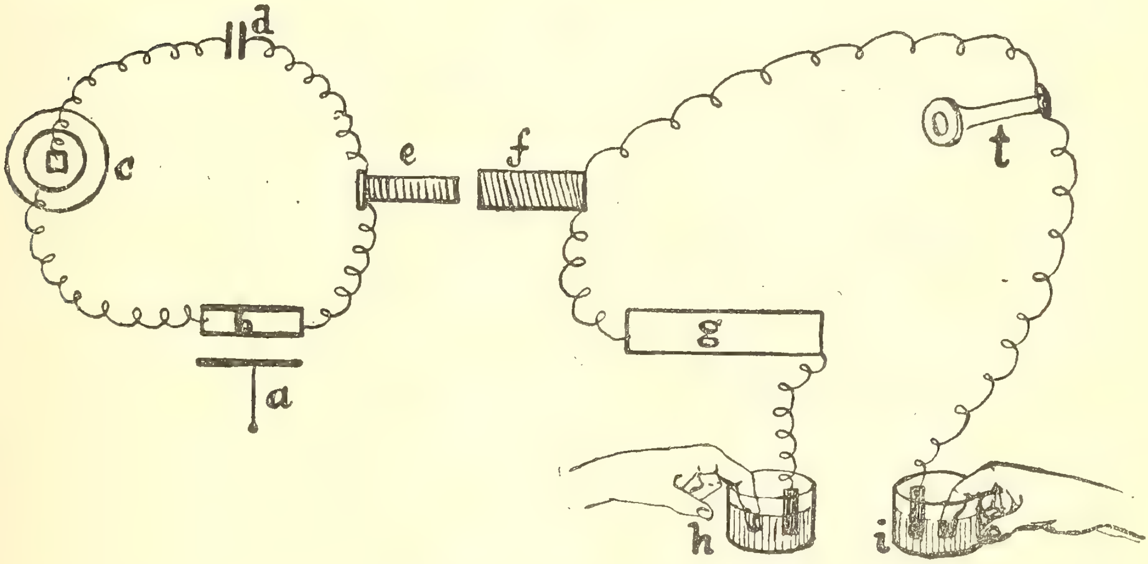
distinction was to be dependent in any degree on mental phenomena, Dr Dubois was perfectly justified in regarding it as a transitional form, because it was a long time after the attainment of the erect posture that his religious, moral, and intellectual faculties became human characteristics. Dr Munro believed that many fossil remains of man were intermediary links which marked different stages in the history of mankind, and the further back such investigations carried them the more Simian-like did the brain-case become. If the geological horizon of the Java man was correctly defined as the borderland between the Pliocene and Quaternary periods, they could form some idea how far back they had to travel to reach the common stock from which men and the anthropoid animals had sprung. The lower races of to-day were also survivals of intermediary links which had been thrown into the side eddies of the great stream of evolution.*

* This paper will be published in full in the author's work—*Prehistoric Problems*—now in the press.

Note on the Sensitiveness of the Skin to Weak Electric Currents, as compared with the Sensitiveness of a Telephone to the same Currents. By John G. M'Kendrick, Professor of Physiology, University of Glasgow.

(Read February 1, 1897.)

On 7th December last, I showed to the Society a method by which the skin might be stimulated by electric shocks from an induction coil corresponding in rhythm and intensity to the notes and chords of music given off by the phonograph. The pressures from the disk of the phonograph act on a variable resistance microphone transmitter, and the current passing through



Description of Diagram.—*a*, phonograph disk; *b*, variable resistance; *c*, Obach's dry cell; *d*, key; *e*, primary, and *f*, secondary coils of induction machine; *g*, resistance box; *h*, *i*, glass beakers, containing salt solution and slips of platinum; *t*, telephone.

the latter also passes through the primary coil of an induction machine. From the secondary coil wires pass to two strips of platinum, immersed each in a beaker containing ·75 per cent.

of salt solution. When the fingers are dipped into the salt solution, and the phonograph is set in motion, thrills are felt by the fingers corresponding to the intensity and rhythm of the music.

Deeming it of interest to have some data regarding this experiment, I have modified it in the way shown in the diagram, and the E. M. F. of the battery employed and the resistances of the various pieces of the apparatus were measured. Into the circuit of the secondary coil, *f*, a resistance box, *g*, and a telephone, *t*, were introduced; and the circuit was completed when the fingers were immersed in the salt solution *h*, *i*. When the phonograph was in action and gave out a well-known military march, the thrills could be felt in the fingers, and at the same time the telephone, which was fixed in a stand at the level of the ear, gave out the music. Resistance was then introduced into the secondary circuit until the "thrills" could be no longer felt, and it was observed that still the telephone could be heard. The measurements were as follows:—

(1) 1 Obach cell used—Q pattern;
Internal resistance, 1·37 ohms;
E. M. F., 1·25 volts.

(2) Resistance of primary of induction coil, 0·183 ohms.

(3) Resistance of secondary of induction coil, ... 726·3 ohms.

(4) Resistance of telephone, . . . 118·0 ohms.

(5) Mean resistance of variable resistance transmitter, ... 21 ohms.

(6) With a finger in each beaker, mean resistance between terminals was . . 50,000 ohms.

When the resistance-box was placed in the secondary circuit of the induction coil, the "thrills" were felt until an extra resistance of 12,220 ohms was introduced. With this resistance the telephone still gave out the music as distinctly as before, and it was not until a resistance equal to about 1,250,000 ohms had been introduced that the telephone ceased to be audible. Thus, variations of current intensity, corresponding to the loudness of the tones of

the music, passed through the fingers and the body of the observer sufficient to excite the telephone, and to cause the latter to give out sounds long after the skin had ceased to respond. In other words, electric currents much too feeble to excite the skin were still capable of acting on a telephone, and the ear could hear the musical sounds emitted by the telephone long after the skin had ceased to feel.

A Research into the Nature of the Nucleins and Paranucleins of the Animal Cell. By T. H. Milroy, M.D., B.Sc.

(Read December 21, 1896.)

(*Abstract.*)

There are two constituents of the cell which have, during the last few years, attracted a great deal of attention, namely, the nucleins and paranucleins.

The nucleins are essential constituents of the cell nucleus, appearing there in different forms of combination. The true nucleins possess at least three characters in common.

In the first place, they have a high percentage of phosphorus.

Secondly, they offer great resistance to the action of peptic digestion.

Thirdly, they furnish nuclein bases on being subjected to the action of weak acids at moderate temperatures (about 100° C.). They may be regarded as representing combinations between a rich phosphorus-holding acid—nucleic acid—and albumin. These combinations may either be of a firm or loose character, that is to say, in the case of the former it is difficult to split off the nucleic acid from the nuclein by the action of hot saturated baryta solutions, while in the latter the decomposition takes place easily.

It seems probable that there are four or more different nucleic acids, according as they give on decomposition certain definite nuclein bases. In other words, each nuclein base may have its corresponding nucleic acid from which it is derived. From this it follows that there may be also different nucleins corresponding to each nucleic acid. As examples, one may take thymus nuclein, which gives in decomposition with weak acids adenin and guanin, while, from the nuclein of the pancreas, only guanin has been obtained. This acid constituent of the nuclein possesses the property of precipitating proteids out of their solutions. It has been regarded as not unlikely that these precipitates might be similar to the natural

nucleins. In order to find out whether this were the case or not, I examined these bodies, which I shall call "artificial nucleins," in different ways.

The first one, which I investigated, was obtained after the following method:—A weak solution of syntonin in 0·25 per cent. hydrochloric acid was prepared in the usual way, and to it a 1 per cent. solution of nucleic acid in water was slowly added until no more precipitate appeared. The artificial syntonin nuclein so prepared was filtered from the excess of fluid, then washed with alcohol and ether, and finally dried in a desiccator. Portions were analysed before and after peptic digestion, and it was found in all cases that the percentage of phosphorus remained fairly constant at about 4 per cent. It was only after very prolonged peptic action that the phosphorus percentage of the undissolved nuclein began to fall, and then only very slightly. A gradual solution of the nuclein, however, took place continuously.

The phosphorus present in the filtrate (*i.e.*, the soluble peptic products) was almost entirely in organic combination, in all probability in the form of albumose nucleins.

This artificial syntonin nuclein was also digested with pancreatic extract for varying periods, with the result that in all cases a rapid splitting up of the nuclein took place, the phosphorus passing in organic combination from the undissolved nuclein to the soluble products of digestion. After five to six hours' pancreatic digestion, the percentage of phosphorus fell to 1·5. An interesting property which these soluble products of digestion possess is that of precipitating albumins and albumoses out of their solutions. This property is due to the presence of an organic phosphorus-holding acid, closely allied to nucleic acid.

Weak alkaline solutions, such as 0·25 per cent. Na_2CO_3, also gradually decompose this nuclein, though not to the same extent as the pancreatic extract.

Hydrochloric acid of the same strength as that which occurs in the gastric secretion is practically without any effect on the nuclein.

The combination between nucleic acid and syntonin is a comparatively firm one. In the same way I have examined combinations between different albumoses and nucleic acid. Such bodies were found to be very soluble when subjected to peptic digestion.

Nucleic acid seems to have the power of uniting with varying quantities of different albumoses, the phosphorus percentage of those bodies being, as a rule, higher than that of the syntonin nuclein.

In order to compare these artificially-formed nucleins with the natural ones, I shall sum up very shortly some of the results derived from investigations which I have made into the nature of the latter class of proteids.

The nuclein present in the thymus gland was found to contain about 4·5 per cent. phosphorus. Even very prolonged subjection (10 hours) to the action of the gastric juice had only the effect of diminishing to a very slight extent this percentage. Here, however, as in the case of the syntonin nuclein, there was a gradual solution of the thymus nuclein, the phosphorus appearing in organic combination among the soluble products of digestion. Trypsin and sodium carbonate (0·25 per cent.) rapidly split up this nuclein, the phosphorus percentage falling after four hours' digestion with pancreatic extract from about 4·5 to 1·8 per cent.

A proteid precipitating body is present among the soluble digestion products.

In the case of the thymus nuclein, the combination between nucleic acid and the albumin is a loose one.

The nucleins of the red blood corpuscles of various birds and also that of the pancreas were examined in the same way.

All are characterised by the resistance which they offer to peptic digestion, and the ease with which the pancreatic extract splits them up.

The combinations between the acid and albuminous radicles are in both cases of a firm description.

The paranucleins can only be defined in rather an indefinite way. They possess a high percentage of phosphorus, which is present in organic combination. We always find the paranuclein united in the tissues to a variable amount of proteid, which is removable by the action of the gastric juice. Examples of these combinations are casein and ovovitellin.

The paranucleins do not offer so great resistance to the action of the peptic ferment as the nucleins. They do not, on decomposition with acids, furnish nuclein bases.

From some (*e.g.*, ichthyulin of the carp's eggs) a cupric oxide reducing body has been obtained, while in the case of others it is absent (casein, ovovitellin).

Just as a nuclein is a combination between nucleic acid and albumen, so a paranuclein may be regarded as a combination between paranucleic acid and albumin.

Unfortunately, we know very little about the acid constituent of the paranucleins. That of the paranuclein of casein has been referred to by Clara Willdenow, Salkowski, and Hahn, though it has never been properly described. Before I refer to the acid constituent of the paranuclein of ovovitellin, it is necessary for me to describe another acid which was formerly regarded as possibly identical with the paranucleic acid of the paranucleins. When a solution of nucleic acid in water is heated for a short time on a water bath, the nucleic acid is split up into nuclein bases, another base called cytosin, and a phosphorus-holding acid —thymic acid. This acid—thymic acid—still retains the proteid precipitating power of the nucleic acid, and is richer in phosphorus than the latter. From analyses which Kossel has made of it, the atomic proportions of nitrogen to phosphorus are as 1·5 : 1, or, according to some of my own, 1·6 : 1; while in all nucleic acids the proportions are always as 3 : 1.

This acid does not give on decomposition nuclein bases, agreeing thus with the acid constituent of the paranucleins. What is the nature of these precipitates which thymic acid causes in syntonin solutions? Are they at all comparable with the natural paranucleins? These bodies I have examined, and shall shortly sum up some of their principal characters. They do not possess so high a percentage of phosphorus as the nucleins, owing to the fact that thymic acid precipitates a greater excess of loosely combined syntonin than nucleic acid does; therefore, on digestion with pepsin, the percentage of phosphorus gradually rises, showing that the excess of syntonin is slowly removed. I may take as an example of many analyses the following:—

Percentage of phosphorus before peptic digestion, .	2·755
After 6 hours' digestion, ...	3·286
„ 16 „	5·045
„ 18 „	4·34

That is to say, first of all, there is a removal of the excess of syntonin, and then after very prolonged action, a splitting up of the paranuclein occurs, shown by the fall in percentage of phosphorus.

Trypsin rapidly splits up this paranuclein.

Both after prolonged peptic and short tryptic digestion, a proteid precipitating body is present in the soluble products of digestion. In order to compare this syntonin-paranuclein with the natural ones, I examined one of the latter, namely, the paranuclein of ovovitellin. It contains about 5 per cent. phosphorus, and is moderately resistant to action of pepsin, but is easily split up on tryptic digestion, or from action of weak alkalies. In all these points it agrees with the syntonic paranuclein.

We may call the organic phosphorus-holding acid, which is split off from this paranuclein by the action of weak alkalies, paranucleic acid. It is distinguished—

First, by its great solubility in cold water;

Second, by its proteid precipitating property;

Third, by the absence of nuclein bases among its decomposition products;

Fourth, by the absence of any reaction with Millon's reagent;

Fifth, by its high phosphorus percentage, 7·97;

Sixth, by the absence of all reactions common to proteids, with the exception of the biuret, which it gives most distinctly.

I have, in conclusion, to thank Professor Kossel of Marburg for his never-varying kindness, attention, and help in difficulties; and especially to Professor Rutherford, who, during my tenure of the Crichton Research Scholarship in Physiology, did everything in his power to assist me with my work.

A grant of £30, which I received from the Dickson Travelling Fund, was of great assistance in allowing me to carry on a research which I was exceedingly anxious to complete.

Theoretical Researches on the Daily Change of the Temperature of the Air. By Dr J. Halm. (With a Plate.)

(Read July 20, 1896.)

I have the honour to lay before the Royal Society a short account of some theoretical researches on the daily change of the temperature of the air, made by me in 1895, and published to a certain extent in the *Nova Acta der kaiserlich Leopoldinisch-Carolinischen Academie der Naturforscher*, Band lxvii., No. 2. Some more recent investigations on the subject, chiefly dealing with the physical point of view of the problem, have not yet been published, but seem to me of such importance as to justify me in presenting a brief sketch of them to the Society in this paper.

The history of the mathematical aspect of the problem can be traced back as far as the middle of the last century, when *Lambert* made the first attempt at investigating the relation between solar and terrestrial radiation and the temperature of the atmosphere. He was succeeded by a great number of famous mathematicians and meteorologists, whose labours, however, though they contributed considerably to our knowledge of certain parts of the question, have not been rewarded, it must be said, with any sufficient result; so that a satisfactory solution of the problem by means of theoretical investigations has been eventually considered impossible, at least in some quarters.

There can be no doubt that, in a problem like this, an immense number of different errors affect the principal conditions of the propagation of heat, which conditions are supposed to be the causes of the observed changes of the temperature. The clouds, which alter the quantity of solar heat radiated towards the surface of the earth, the aqueous vapour continually changing its amount in the atmosphere, the convection currents carrying heat from the soil to the upper layers of the air, and allowing colder atmospheric elements to come into contact with the thermometer, the wetness

of the soil, which alters its thermal capacity, as well as the quantity of heat lost by the change of water into vapour;—all these phenomena, which must be taken into account, add to the difficulty of the investigation. The following sketch, however, may perhaps suffice to show how, by comparatively simple considerations, the effect of all the disturbances just mentioned may find expression in the mathematical solution of the problem.

The first principal question we have to answer will be: By what means does the lowest layer of the atmosphere, the temperature of which is recorded by our thermometrical instruments, receive or lose heat? There can be no doubt that the chief cause of every change of temperature within this layer must be found in the communication of heat from the surface of the soil to the atmosphere. It is obvious that a loss of heat from this surface to the colder parts of the atmosphere is, to a certain extent, compensated by a contribution of heat from the hotter parts. There must be a continuous interchange of heat between the soil and a certain quantity of the atmosphere, of which we know neither the height nor the distribution of temperatures within it. To simplify the question, we suppose that there is no *direct* radiation from the soil into space; but we are not necessarily bound to this assumption, because it is obvious that any amount of heat supposed to be radiated into space may just as well be assumed to be radiated against a further imaginary layer of the atmosphere, so long as we can dispose of the density and temperature of this layer.

Now, let us take a certain element of mass, dm, of the atmosphere at any distance from the soil. Let $\phi(\lambda)$ be the coefficient of radiation of the soil against this element, t_k its temperature, and t' the temperature of the earth's surface. We know, from Newton's law, that the quantity of heat radiated during the infinitely small space of time, dz, from the soil towards the element, is given by the formula:

$$dv' = -\phi(\lambda)dm(t' - t_k)dz \, *$$

* This formula obviously includes the case of conduction between the soil and the first elements of the atmosphere too—provided we make no assumptions on the nature of the function $\phi(\lambda)$. If, in what follows, we simply speak of radiation between soil and air, we always tacitly refer to the probable existence of such conduction.

The coefficient of radiation is not the same for different elements; it will certainly depend on their distance from the source of heat. Taking n different elements, we may express the corresponding coefficients by

$$a_1\lambda_0, a_2\lambda_0, \ldots \, a_n\lambda_0,$$

where $a_1, a_2 \ldots a_n$ are numbers different from each other. The quantities of heat given to these elements by the soil are consequently

$$-a_1\lambda_0 dm(t'-t_1)dz \,; -a_2\lambda_0 dm(t'-t_2)dz \,; \ldots \, -a_n\lambda_0 dm(t'-t_n)dz,$$

and are obviously identical with the quantities of heat radiated to $a_1, a_2 \ldots a_n$ elements of mass with the *same* coefficient of radiation, λ_0.

Applying this consideration to the whole of that part of the atmosphere which is influenced by the radiation of the soil, we conclude that we are allowed to divide the atmosphere into an unknown number of layers with unknown temperatures, but the *same* coefficient of radiation, λ. The thickness of these layers naturally depends on our assumption of the element dm, and the nature of the function $\phi(\lambda)$. But, whatever be the differences between these, we can always assume an infinitely large number of sufficiently thin layers.

Now, suppose there be $2n$ layers, n being a large number, $t_1, t_2 \ldots t_{2n}$ their respective temperatures, we obtain the total amount of heat contributed by the soil to the whole atmosphere:

$$-2n\lambda(t'-\mathrm{T})dz,$$

where T is the arithmetic mean of the temperatures of all the layers.

Thus, we know that, whatever be the distribution of temperatures and densities throughout the atmosphere, the amount of heat given by the earth's surface will only depend on the mean temperature, T, the temperature of the soil, t', and the coefficient of total radiation, $2n\lambda$. It is evident that a supposed ideal atmosphere, with the same mean temperature, T, and the same coefficient of radiation as the real one, would produce exactly the same effect on the change of temperature in the earth's surface.

Now, let us assume the temperature of the lowest layer of the new atmosphere to be identical with the observed temperature of the real one, we do not alter by this supposition the conditions of heating this layer. For we can easily prove that any change of heat in it cannot but depend on its own temperature, the temperature of the soil, the mean temperature, T, and its coefficient of total radiation—factors which are supposed to remain unaltered by the new supposition.

We again assume the substituted atmosphere to contain $2n$ layers, the temperatures of which are as follows:

$$T+n\Delta\,;\ T+(n-1)\Delta\,;\ T+(n-2)\Delta\,;\ \ldots\ldots\ T+\Delta\,;\ T-\Delta\,;\ \ldots\ldots\ T-(n-2)\Delta\,;\ T-(n-1)\Delta\,;\ T-n\Delta,$$

where Δ is an extremely small increment of temperature. All these layers must be supposed to have the same coefficient of radiation, λ, and, besides, there exists the condition

$$T+n\Delta=t,$$

where t is the observed temperature of the lowest layer of the atmosphere.

Considering two of these layers, of which the temperatures are $T+(n-k)\Delta$ and $T-(n-k)\Delta$, we know that the quantities of heat received from the earth's surface are respectively

$$-\lambda\{t'-T-(n-k)\Delta\} \quad\text{and}\quad -\lambda\{t'-T+(n-k)\Delta\}.$$

But apparently the sum of these quantities is not altered by assuming the first layer to have the temperature, $T+n\Delta=t$, and the second one, $T-n\Delta$. Thus we come to the general conclusion, that we may assume the radiation of the soil to be directed towards two masses of air with the respective temperatures, $T+n\Delta$ and $T-n\Delta$, in *all their elements*, each of which masses has the coefficient of radiation, $n\lambda$; and that the total amount of this radiation is equal to the whole radiation against the real atmosphere.

Of course we know nothing about the temperature, $T-n\Delta$, of the upper limit of our supposed atmosphere. But it has been shown, with some approach to certainty, by the very few observations made, as well as by theoretical considerations, that, at a

certain distance from the soil, the daily fluctuation of temperature disappears. Assuming the upper limit of that part of the atmosphere which receives heat from the soil to extend as far as this critical distance, we naturally have the condition—

$$T - n\Delta = u = \text{const.}$$

So far, we have the following result: *The total amount of heat given by the soil to the whole atmosphere by radiation (as well as by conduction) can be considered as directed against two masses of air with the same coefficient of radiation, one of which masses has the temperature of the lowest layer,* t, *the other a constant temperature,* u.

It is important to state that this result is obtained without any assumption as to the distribution of temperatures *within* the atmosphere. Certainly, a heated element of air cannot remain in its position; we know that heat is carried upwards by continual convection currents, which produce, on the other hand, a horizontal flow of air near the earth's surface. But it seems quite evident from what we deduced before that this perpetual circulation of heat can only affect our result so far as it influences the temperature of the upper and lower *limits* of the part of the atmosphere under consideration,—that is to say, the temperatures t and u. Now, however considerable the influence of the upward directed currents be on the temperatures of the single layers, there can be no doubt that at a comparatively small height they fade away, and that therefore we have not the slightest reason for altering our supposition as to the constancy of the temperature u. How, on the other hand, the horizontal currents affect the changes of the temperature t from one moment to another must be considered at a later part of this paper.

At first sight, it would appear rather unreasonable to have two masses of air with *different* temperatures, without supposing that there exists radiation between these masses themselves. No doubt there will be. Let us suppose the mass of air at temperature t radiating heat against the mass of air at temperature u,—that is to say, contributing one part of that heat which is necessary to maintain the constancy of the temperature u. Consequently, the whole quantity of the required heat being necessarily the same in

all cases, the radiation of the soil against the mass of air at temperature u must be less than it was under the assumption that *no* radiation exists between the two masses. But, on the other hand, the mass of air at temperature t arriving at a lower temperature by its own radiation, its power of absorption from the soil will be increased at the same time. Now, we know that the radiating (absorptive) power against the earth's surface is equal for both masses. Thus, the same quantity of heat radiated by one and absorbed by the other will affect the earth's radiation in each case in exactly the opposite way. Hence, it follows directly, first, that the total amount of heat given by the soil to the air is the same, whatever be supposed as to the communication of heat between the elements of the atmosphere themselves; and secondly, in making the assumption that all the heat required for maintaining the constant temperature, u, is spent by the earth's surface, we are bound, on the other hand, to give the soil the whole advantage of the radiation coming from the mass of air at temperature t.

If we denote by dw' the quantity of heat radiated by the soil during the time dz, by dw that radiated by the mass of air at temperature t, we may represent our result by the following equations:—

$$\frac{dw'}{dz} = -n\lambda(t'-u) - n\lambda(t'-t)$$

$$\frac{dw}{dz} = -n\lambda(t-t').$$

Apparently these equations are true only so long as there are no other sources of heat except the soil and the atmosphere, which is the case during the night. How they are affected by the solar radiation during the day shall be pointed out afterwards.

The equations in their present form give the total change of *heat* in both the earth's surface and the air. To find the change of *temperature* we must introduce some further considerations. What we really know by the fundamental law of the relation between the radiation and the absorptive power of a body only refers to the whole quantity of heat radiated or absorbed. But, unfortunately, we do not as yet know what part of this whole quantity is absorbed by a distinct element at the surface of the body. Taking

such an infinitely small element, we are sure that only a comparatively small part of the whole radiation received by the surface will be actually absorbed within the element, which allows the larger part to enter the neighbouring elements, where it is again partly absorbed, and partly carried to deeper layers according to the laws of conduction. Owing to the fact that radiation of a solid like the earth must be a mere action of the surface, the question we have to consider is, How much heat is lost or gained by an infinitely small element of this surface? All the other heat passing through this element without absorption is of no importance to our question, because it does not change the temperature, t', of the surface.

The general problem, how much of a given intensity of radiation is absorbed by a certain element of a resisting medium, was first solved by *Bouguer*, who gave a formula for representing the extinction of light in the atmosphere. There is no obvious reason why the same considerations should not apply to the radiation of heat. Let us call α the coefficient of absorption for a given substance, ρ its density, dV the volume of an infinitely small element, σ the intensity of radiation falling upon the surface of this element, $d\sigma$ the quantity of radiation absorbed, then we have the well-known formula

$$d\sigma = -\alpha\sigma\rho dV.$$

From this formula we learn that two different bodies with the same absorptive power, but different densities, absorb different quantities of radiation in equal elements of their surfaces, which are represented by the relation:

$$\frac{d\sigma_1}{d\sigma_2} = \frac{\rho_1}{\rho_2}.$$

As the absorptive power of the soil against the two masses of air must necessarily be the same as the power of radiation of these two masses against the soil, and *vice versa*, this last relation must exist between equal elements of volume of the soil and the air at their surfaces.

Now we have the two equations

$$\frac{dv'}{dz} = c_1\rho_1 dv . \frac{dt'}{dz} \text{ and } \frac{dv}{dz} = c_2\rho_2 dV\frac{dt}{dz},$$

dv', dv denoting the changes of heat in equal elements of volume of the surface and the mass of air at temperature t, c_1 and c_2 being the specific heat of the soil and the atmosphere for unit of weight. Our differential equations for the changes of *temperature* in each element will therefore be:

$$c_1\rho_1 dV\frac{dt'}{dz} = -d\sigma_1(t'-u) - d\sigma_1(t'-t)$$

$$c_2\rho_2 dV\frac{dt}{dz} = -d\sigma_2(t-t'),$$

$d\sigma_1$ being the quantity of heat absorbed, or radiated, by an element of volume of the earth's surface from, or to, each of the two masses of air, the difference of temperature being 1°; $d\sigma_2$ the same for an equal element of volume of the air. Applying the above-mentioned relation:

$$d\sigma_1 = \frac{\rho_1}{\rho_2}d\sigma_2 \text{ and putting } \frac{d\sigma_1}{\rho_1 dV} = \sigma, \text{ we obtain}$$

$$\left.\begin{aligned} c_1\frac{dt'}{dz} &= -\sigma(t'-u) - \sigma(t'-t) \\ c_2\frac{dt}{dz} &= -\sigma(t-t') \end{aligned}\right\}$$

which may be considered as the fundamental equations of our problem.

Now, a considerable simplification of these equations arises from the curious fact that the average values of the specific heat of the ordinary soil and the atmosphere are practically the same. The well-known value for the air by *Régnault* is 0·238, which has, of course, to be slightly enlarged on account of the presence of aqueous vapour in the atmosphere. On the other hand, very careful observations by *Pfaundler* have proved that, for the mineral part of the earth's crust, whatever be its chemical constitution, we may assume the well-determined value 0·20. But, in most cases, a certain amount of humus will increase this number, and he has found that, for average conditions, the value of 0·25 to 0·28 will be the proper quantity, which is indeed very close to the value for the air under ordinary circumstances. No doubt the amount of water in the soil will still influence its thermal capacity;

but, considering the fact that only the outmost superficial parts contribute to the radiation, and that these very soon get dry by the perpetual process of evaporation, this influence cannot be very great. However, it must exist, and it is very satisfactory to find that the observations, as we shall see, show this small difference in the theoretical equations in an extraordinary manner. On the other hand, we must also take into account that most of our meteorological stations are near houses, roads, etc., the specific heat of the stonework of which is certainly less than the average, and not exceeding 0·20.

Hence, if it is permissible to speak of an average value of the specific heat of the soil at all, we have strong reasons to suppose it practically identical with that of the atmosphere. So, putting

$$c_1 = c_2 \text{ and } \frac{\sigma}{c_1} = h,$$

we arrive at our final system of equations:

$$\left.\begin{aligned} \frac{dt'}{dz} &= -h(t'-u) - h(t'-t) \\ \frac{dt}{dz} &= -h(t-t') \end{aligned}\right\}$$

The equations in this form are not new; they were given more than twenty years ago by *A. Weilenmann* in the *Meteorologische Zeitschrift* for 1873, and especially in a most interesting paper, "Ueber den taeglichen Gang der Lufttemperatur zu Bern," an attempt to theoretically represent the curve of the air temperature during the night. But, unfortunately, he did not succeed in giving a sufficient explanation of his mathematical terms, nor was he able to extend the theory to the principal and more interesting part of the daily curve, that between sunrise and sunset. Nevertheless, great merit is due to him for having first propounded these equations, which not only represent practically the whole daily curve of temperature after a proper application of the law of solar radiation in a most perfect way, but give answer to questions concerning some conditions of our atmosphere till now unsolved, as well as to the last and most important theories on the periodicity of solar heat.

From the two simultaneous equations just mentioned we derive, by elimination of t', a differential equation of the form:

$$\frac{d^2(t-u)}{dz^2} + 3h\frac{d(t-u)}{dz} + h^2(t-u) = 0\,,$$

the integral of which is

$$t = u + \mathfrak{c}_1 e^{\lambda_1 z} + \mathfrak{c}_2 e^{\lambda_2 z},$$

where λ_1 and λ_2 are the roots of the quadratic equation

$$\lambda^2 + 3h\lambda + h^2 = 0$$

$$\lambda_1 = -0{\cdot}382\,h\,; \quad \lambda_2 = -2{\cdot}618\,h\,.$$

By a careful investigation of the temperatures at several places, *Weilenmann* proved empirically that the arbitrary constant $\mathfrak{c}_2$ is zero in all cases, so that we have the particular integral

$$t = u + \mathfrak{c}\,e^{-0{\cdot}382\,hz} \quad (z \text{ in units of the hour}).$$

From the theoretical point of view, this equation is of great importance, giving, as it does, a numerical value of the coefficient of radiation h, which was really found by Weilenmann to be a constant for all the places considered and all conditions of the atmosphere, the average value being 0·375. This numerical value seems to represent perfectly the observations at all the stations on which I have founded my computations, leaving an error, which is certainly *below* the mean accidental error of the observations.

Let us now consider the important change of conditions produced by the solar radiation during the day. It is well known that the sun's rays passing through the atmosphere suffer a considerable loss of their directly radiated light and heat by the absorptive and reflective power of the air. But it would be quite insufficient to introduce only this direct part of solar heat into our problem without noticing those enormous outstanding portions of solar radiation which at last reach the earth's surface after being deflected once, or even oftener, from their direct course by the interfering particles of the air. We therefore bring to our aid a careful investigation of both the direct and diffused radiation of solar heat which is found in an elaborate discussion due to *W. Zenker*, entitled "Sur la distribution de la chaleur sur le globe." In this paper the

author deduces from the observations of *Langley* * a formula, founded on the ingenious mathematical researches of *Clausius*, giving the total amount of solar radiation which *enters* the earth's surface at any altitude of the sun. This formula may be represented in general terms by the equation:

$$I = A\cos Z - b,$$

where I is the total radiation arriving at the surface, A and b calorimetric constants, and Z the zenith distance of the sun. *Zenker's* investigations being the best and most extensive known to me, I accepted his final formula for the following deductions:—

We know that if a body be exposed to the radiation of a *constant* source of heat, there will be produced—theoretically after an infinite, but practically after a comparatively short time—a state of thermal equilibrium in the body, it receiving from the source exactly the same quantity of heat as it transfers in the same time to its neighbouring elements by radiation or conduction. After once having established this final state, there is no more change in the temperatures of the body and its neighbourhood as long as we assume the source of heat to be constant. If we, for instance, could suppose the sun, a short time after having risen, to remain strictly in a fixed position, we would see that shortly after that moment the soil as well as the lowest elements of the atmosphere had assumed a constant temperature, to be maintained during the whole time during which the sun is fixed in this constant position. But now, let the sun's altitude increase for an infinitely short space of time, then naturally the earth's surface receives more radiation at the end of that time than at the beginning, and it is obvious that the increase of its temperature must be proportional to the difference of solar radiation in the two positions. So, by adding this term to our former equations, we obtain

$$\left.\begin{aligned} \frac{dt'}{dz} &= -h(t'-u) - h(t'-t) + K\frac{d\cos Z}{dz} \\ \frac{dt}{dz} &= -h(t-t') \end{aligned}\right\}$$

* *Researches on Solar Heat and its Absorption by the Earth's Atmosphere.* A Report of the Mount Whitney Expedition.

where K is a constant depending on the solar radiation as well as the thermal capacity of the soil. Eliminating t', we arrive at the differential equation of the second order

$$\frac{d^2(t-u)}{dz^2} + 3h\frac{d(t-u)}{dz} + h^2(t-u) = -h\mathrm{K}\cos\phi\cos\delta\sin\zeta\,,$$

and consequently at the general integral

$$t = u + c_1 e^{-0{\cdot}382hz} + c_2 e^{-2{\cdot}618hz} + a\cos\phi\cos\delta\{(1-h^2)\sin\zeta + 3h\cos\zeta\}$$

$$a = \frac{h\mathrm{K}}{1+7h^2+h^4},$$

ζ representing the sun's hour angle, δ his declination, and ϕ the latitude of the place of observation.

Obviously we have to take this integral between the limits $-\tau$ and $+\tau$, τ being the semidiurnal arc of the sun, because our equations can only refer to points within this interval. Besides, it can be easily proved that both the arbitrary constants have to disappear. This may be inferred immediately from what we said about the constancy of temperature at constant radiation. Hence we conclude that the integral representing the change of temperature during the day must have the form:

$$t = t_{-\tau} + a\cos\phi\cos\delta\Big[(1-h^2)\sin\zeta + 3h\cos\zeta\Big]_{-\tau}^{\zeta},$$

$t_{-\tau}$ being the temperature of air at any moment near sunrise. We may write this equation in a simpler way by putting

$$(1-h^2)\cdot a = \alpha\sin v$$
$$3h\cdot a = \alpha\cos v$$
$$t_0 = t_{-\tau} - \alpha\cos\phi\cos\delta\cos(t+v)\,,$$

whence we derive

$$t = t_0 + \alpha\cos\phi\cos\delta\cos(\zeta - v)$$

The angle v being determined by the relation tang $v = \frac{1-h^2}{3h}$, the time of maximum of temperature can be found by introducing the above given value $h = 0{\cdot}375$ into this equation. We find $v = 37^\circ{\cdot}4 = 2^{\mathrm{h}}\ 29^{\mathrm{m}}$ p.m.; a value which agrees perfectly with the well-known average epoch of maximum for continental climates derived directly from observations.

I think this is the proper place to make a few remarks about the influence of horizontal convection currents on the temperature of the air. We know that the principal difference in the daily curves of temperature between continental and maritime climates is due to the periodic currents carrying colder air from the sea and mixing it with the more heated elements over the ground. Apparently the loss of heat produced by such a mixture will be greatest when the difference between the temperatures of the heated air and the current is greatest,—that is to say, at the time of maximum of the air temperature. Denoting this moment by v', and putting the quantity of temperature lost by convection $= -dk\cos(\zeta - v')$, we have in this case the following differential equations:—

$$\left.\begin{aligned} \frac{dt'}{dz} &= -h(t'-u) - h(t'-t) + \mathrm{K}\frac{d\cos \mathrm{Z}}{dz} \\ \frac{dt}{dz} &= -h(t-t') - dk\cos(\zeta - v') \end{aligned}\right\}$$

and consequently the integral

$$t = t_0 + a\cos\phi\cos\delta\cos(\zeta - v) - da\cos(\zeta - v - v'),$$

which can easily be proved to show a *decrease* of the daily *range*, and a time of maximum *earlier* than under the conditions of continental circumstances, features well known to be peculiar to places near the coasts.*

* I have to correct a mistake in this part of the paper already published, caused by an erroneous theoretical consideration, which, however, does not alter the result derived from the integral.

At stations where the thermometer is too much protected by houses, etc., and where the sun's rays have not sufficient access to the neighbouring ground, we generally observe a diminution of the daily range connected with a retardation of the time of maximum temperature. It is not difficult to find the cause of these phenomena—first, in the diminution of solar radiation acting upon the neighbouring soil; and, secondly, in the continuous currents of hotter air, which are caused by the upward direction of the currents on the roofs of the houses, and which mix with the colder air near the protected position of the thermometer. Hence we have to alter the above equations by taking a smaller value of K, and changing the sign of the expression $dk\cos(\zeta - v')$. On a much larger scale the same phenomenon appears in mountainous countries, where currents of heated air flowing *up* a valley during day-time give the same peculiar character to the daily curve of its temperature.

No doubt, we also have similar currents at continental stations, and they are distinctly shown by different temperature curves at different places, according as the direction of the wind is from a hotter or cold quarter. But in this case it is quite impossible to pronounce a distinct law about such currents, the probability of their carrying air from colder places being generally as great as the contrary, in a theoretical point of view. Nevertheless, in every instance where a distinct convection current influences the problem, the mathematical explanations just given will suffice to represent the peculiarities of the daily curve.

Our deductions, so far, would be correct on the supposition of a perfectly clear sky,—that is to say, in the absence of visible clouds, as well as of invisible aqueous vapour in the atmosphere. Unfortunately, we are assured by actinometric observers, that even on so-called bright and cloudless days the solar radiation is subject to considerable variations, most probably caused by the continuous changes in the amount of atmospheric moisture. The observations of *Crova* at Montpellier show that, for instance, in the summer the intensity of solar heat at noon is about a third of its whole amount smaller than it ought theoretically to be, even on days which the eye judged as exceedingly bright. Now, *Langley*, on whose labours these considerations, as we have said, are principally founded, had chosen a place for his observations, the atmosphere of which was very much drier than that of the average places we have to deal with; so that the above formula for solar radiation, if applied to ordinary conditions, has to be considerably corrected on account of the discrepancies just mentioned.

The best method of studying this remarkable influence of the daily changes of the moisture seemed to be to carefully investigate the daily curve of cloudiness, which had the following result:—In the winter there is a distinct decrease of clouds from the morning to the afternoon, the minimum being at about 3 o'clock in the evening; in the summer, on the contrary, we have a maximum of clouds very near the culmination of the sun; while in spring and autumn, we observe two epochs, when there is no apparent daily change at all. These three different features, which are very characteristic, and which we believe to give a correct account of the changes of aqueous vapour at the same time, correspond in a remarkable manner to synchronous variations of

solar radiation, so that the change of the temperature of the soil during the instant dz, caused by the sun's *real* radiation, may be represented by a formula—

$$dt' = \{ -\mathrm{K} \cos \phi \cos \delta \sin \zeta + d\mathrm{K} \sin (2\zeta - u) \} dz,$$

where K is again the above-mentioned solar constant, ζ the sun's hour angle, dK a quantity due to the influence of the daily change of clouds and moisture, and u the time of its maximum (dK positive) or minimum (dK negative). On introducing this term into our standard equations, we obtain—

$$\left.\begin{aligned} \frac{dt'}{dz} &= -h(t'-u) - h(t'-t) - \mathrm{K}\cos\phi\cos\delta\sin\zeta + d\mathrm{K}\sin(2\zeta - u) \\ \frac{dt}{dz} &= -h(t-t') \end{aligned}\right\}$$

whence we derive the following integral :—

$$\left.\begin{aligned} & t = t_0 + a\cos\phi\cos\delta\cos(\zeta - v) - da\cos(2\zeta - u - w) \\ & a = \mathrm{K}\cdot\frac{h}{\sqrt{1+7h^2+h^4}}; \ da = d\mathrm{K}\,\frac{h}{\sqrt{16+28h^2+h^4}} \\ & tgv = \frac{1-h^2}{3h}; \ tgw = \frac{4-h^2}{6h} \end{aligned}\right\}$$

This is at last our final equation for representing the daily change of the air temperature from sunrise to sunset. We notice that there is no arbitrary constant, that every parameter bears its distinct theoretical significance, and can be determined directly from the observations. I need scarcely say that only stations at a distance from the sea can be considered, and even then we have to exclude in northern latitudes the extreme winter months, when the snow and the frozen water on and within the soil alter the physical conditions of our problem altogether.

Let us first consider the constant of radiation h. It can be derived from the observations by means of the angle v. We have—

$$h = -\frac{3}{2}\operatorname{tang} v + \sqrt{\frac{9}{4}\operatorname{tang}^2 v + 1},$$

and by this equation I have derived from five trustworthy stations, for the eight months from March to October, the following values :

0·363, 0·363, 0·370, 0·378, 0·366, 0·376, 0·385, 0·388 ; Mean : 0·374,

almost identical with that found by *Weilenmann* from night observations. There is a comparatively very small, but distinct dependence on the cloudiness, which, by means of the preceding fundamental considerations, can easily be proved to be caused by differences in the specific heat of the soil in clear and cloudy weather.

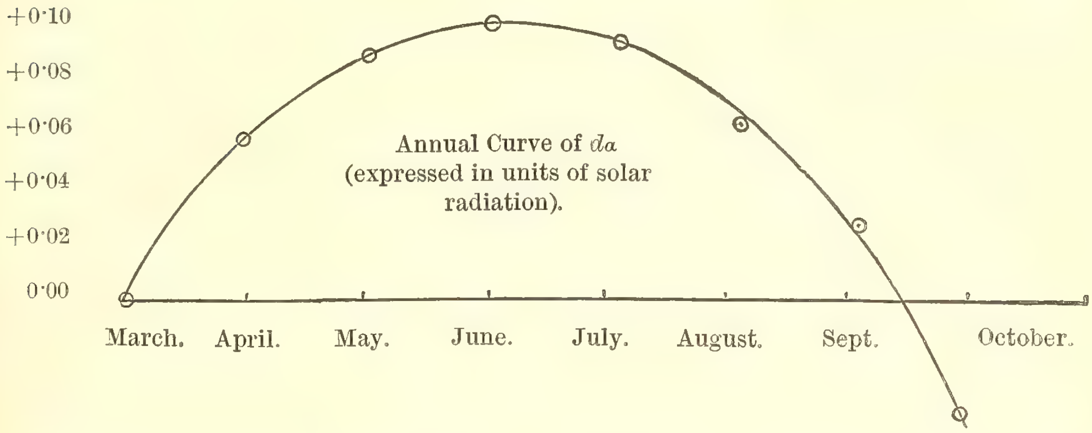
The quantity $d\alpha$ must show a yearly range, having one maximum in midsummer and one minimum in winter; twice near the equinoxes passing through zero. In every month there is a constant proportion between this quantity and the term for the mean solar radiation, $a \cos \phi \cos \delta$, so that we are able, by studying the curves at various stations, to give average factors expressing this quantity in units of solar radiation. The angle u, which expresses the epoch of maximum or minimum of moisture and cloudiness, must indicate in the months from April to September, a time very close to the culmination of the sun; and from October to March, a time approximating to that of the maximum temperature, in accordance with the direct observations and the theory of the daily change of cloudiness and absolute humidity of air.

From fifteen continental stations between 45° and 65° northern latitude, the daily curves of temperature of which were published by *H. Wild* in his standard work *Die Temperaturverhältnisse des russischen Reiches*,* I obtained the following values of $d\alpha$ and the angle u for the months March to October, which show the perfect agreement between the theory of temperature and the directly observed daily changes of cloud and atmospheric moisture.

Month.	$d\alpha$.		Time of Max. or Min. of Moisture.	
March, .	0·000	$a \cos \phi \cos \delta$,	...	
April, .	+0·060	,,	12.2 P.M.	Maximum.
May, .	+0·090	,,	11.9 A.M.	Maximum.
June, .	+0·102	,,	11.9 ,,	Maximum.
July, .	+0·097	,,	12.0 Noon	Maximum.
August, .	+0·069	,,	12.1 P.M.	Maximum.
September,	+0·031	,,	2.2 ,,	Maximum.
October,	−0·033	,,	2.5 ,,	Minimum.

* *Supplementband zum Repertorium für Meteorologie*, St Petersburg, 1881.

We learn from this curve that the maximum of da occurs exactly at the time of the summer solstice. We should have a corresponding minimum in December, but it would be unsafe to follow out our theory for the winter months in these northern latitudes without further careful investigations into the new conditions introduced by the presence of snow and ice on the surface



of the ground. Nevertheless, we have found at all the various places considered very distinct indications of that minimum between December and January. There are two points in our curve, March 15 and October 1, where da becomes zero; and it is indeed a very remarkable fact that the daily curves of temperature at both terms are fully represented by our first simple equation—

$$t = t_0 + a \cos \phi \cos \delta \cos (\zeta - v).$$

Finally, we come to the most important part of our question, the constant of solar radiation a. Theory requires that a must be a constant at all seasons (except winter) and for all places at a (comparatively small) distance from the sea under a given state of cloudiness, both the thermal capacity and the absorptive power of the soil being supposed to have the average values. The amount of heat radiated by the sun being, of course, a function of the absolute state of cloudiness, I considered, by means of a great number of observations selected over the whole scale of clouds ordinarily used, the question, by what function of the cloudiness the sun's heat may be represented, arriving at the remarkable result that for all the stations considered the simple linear function

$$a_n = (1 - \beta n) a_0 ; \ \beta = 0{\cdot}080$$

seemed quite sufficient to practically represent the facts, α_0 denoting the sun's energy when the sky is clear, n the amount of cloud on the scale 0 to 10. Theoretically, we should perhaps expect an exponential function of the form

$$a_n = a_0 e^{-\beta n},$$

and it is not at all impossible that this formula may correctly represent the facts considering the conditions of *heat.* But, assuming the amount of water in the soil to be in some way proportional to the cloudiness, we have certainly to deal with a change in the specific heat of the earth's crust, by which obviously the logarithmic curve, applied to *temperatures*, must be bent more and more towards a straight line.

It was found convenient to derive the value of the solar constant for a mean state of cloudiness equal to 5·0, and the mean latitude of the places considered 50° north. So, if we call α_m the solar constant under these conditions, a_n the numerical coefficient derived from the daily curve of a certain place

$$a_n = \alpha_n \cos\phi \cos\delta,$$

we have the equation

$$\alpha_m = a_n \sec\delta \frac{\sec\phi}{\sec 50^\circ} \frac{1-5\beta}{1-n\beta},$$

n being the mean state of cloudiness at the time considered.

Now, from the above quoted work by *H. Wild* were taken 20 different stations in various latitudes, from which were formed three normal places with the respective latitudes 59°·4, 51°·3, and 42°·9. For every month from April to September a_m was derived from that part of the hourly curve between sunrise and sunset, the result being the following:—

If we combine the three places in every month separately, we obtain:

	April.	May.	June.	July.	Aug.	September.
$\alpha_m =$	6°·23	6°·23	6°·27	6°·31	6·43	6°·23 Centigrade.

If, on the other hand, we take the mean of the six months for each of the three stations separately, we arrive at the values:

Latitude.	a_m.
59°·4	6°·45 Centigrade.
51°·3	6°·21 „
42°·9	6°·19 „

There is apparently a distinct increase of the solar constant towards the north. But it is a well-known fact, from direct actinometric observations, that indeed the solar radiation becomes more intense in northern latitudes, most probably a result of the lower mean state of atmospheric moisture. Thus we may undertake to assert that our result shows that a determination of the solar constant from observations of the daily change of temperature may be relied on with confidence. We may even go so far as to say that these observations may be used for testing the important discoveries previously made as to the periodic character of the solar radiation.

After the elaborate investigations of this question by the late Astronomer-Royal for Scotland, *Dr C. Piazzi Smyth*, founded on the observations of the rock thermometers at Calton Hill, and similar researches of *Prof. Stone* at the Cape, and others, there can be no doubt that a cyclical wave of the sun's heat must really exist, the period of which is synchronous with the eleven-year period of sun spots.

Unfortunately, we have not yet at our disposal a sufficient number of stations with hourly observations. But we can easily show that all the places where only two or three observations a day were made—of which, indeed, an enormous number are available—can also be used for determining the solar constant.

We have seen how regular the annual curve of the quantity da has proved, and, at the same time, how constant is the angle u; now, by introducing the above given values of both these parameters and the value of the angle v determined by the equation $\operatorname{tang} v = \frac{1-h^2}{3h}$, we reduce the number of unknowns from 5 to 2, that is to say, the constant temperature t_0 and the solar constant a_n. After some particular considerations about the curve of temperature

near sunrise, where naturally it does not follow the theory for some time, for reasons which may be deduced without difficulty from the theoretical considerations already given, we arrive at a formula,

$$t = \text{Min.} + a \,.\, a_n \cos\phi \cos\delta\,,$$

a being a numerical quantity tabulated for each hour of the day from sunrise to sunset. Now, these last two epochs also belong to the night curve, which we know to be represented by the equation :

$$t = u + \mathfrak{c} \,.\, e^{-0{\cdot}382hz},$$

so that the two unknowns of this last equation can be determined by the minimum temperature and the solar constant. Hence we arrive at the result that the temperature at every hour of the *whole* day may be represented by the above-mentioned formula :

$$t = \text{Min.} + a a_n \cos\phi \cos\delta\,.$$

It would far exceed the scope of this paper should I attempt to introduce the tables of the quantity a, which I hope to publish *in extenso* at a future time.

Now, having, for instance, three times of observations, the most usual being morning, afternoon, and evening, let us denote the observed temperatures by t_1, t_2, t_3; the respective values of a by a_1, a_2, a_3. Then we have the following relation :

$$a_n = \frac{2t_2 - t_1 - t_3}{2a_2 - a_1 - a_3} \sec\phi \sec\delta\,.$$

In this way we obtained, by discussion of the observations of a great number of stations in Russia and Austria-Hungary, where the observing hours were 7 A.M., 1 P.M., 9 P.M., and 7 A.M., 2 P.M., 9 P.M. respectively, the following values of a_m for the mean latitude 49° :

	April.	May.	June.	July.	Aug.	September.
$a_m =$	6°·08	6°·19	6°·11	6°·19	6°·15	6°·10 Centigrade.

On the other hand, by taking again the mean of the six months in each year, we arrived at values, which we have given in the affixed curves, separately for each of the combinations mentioned

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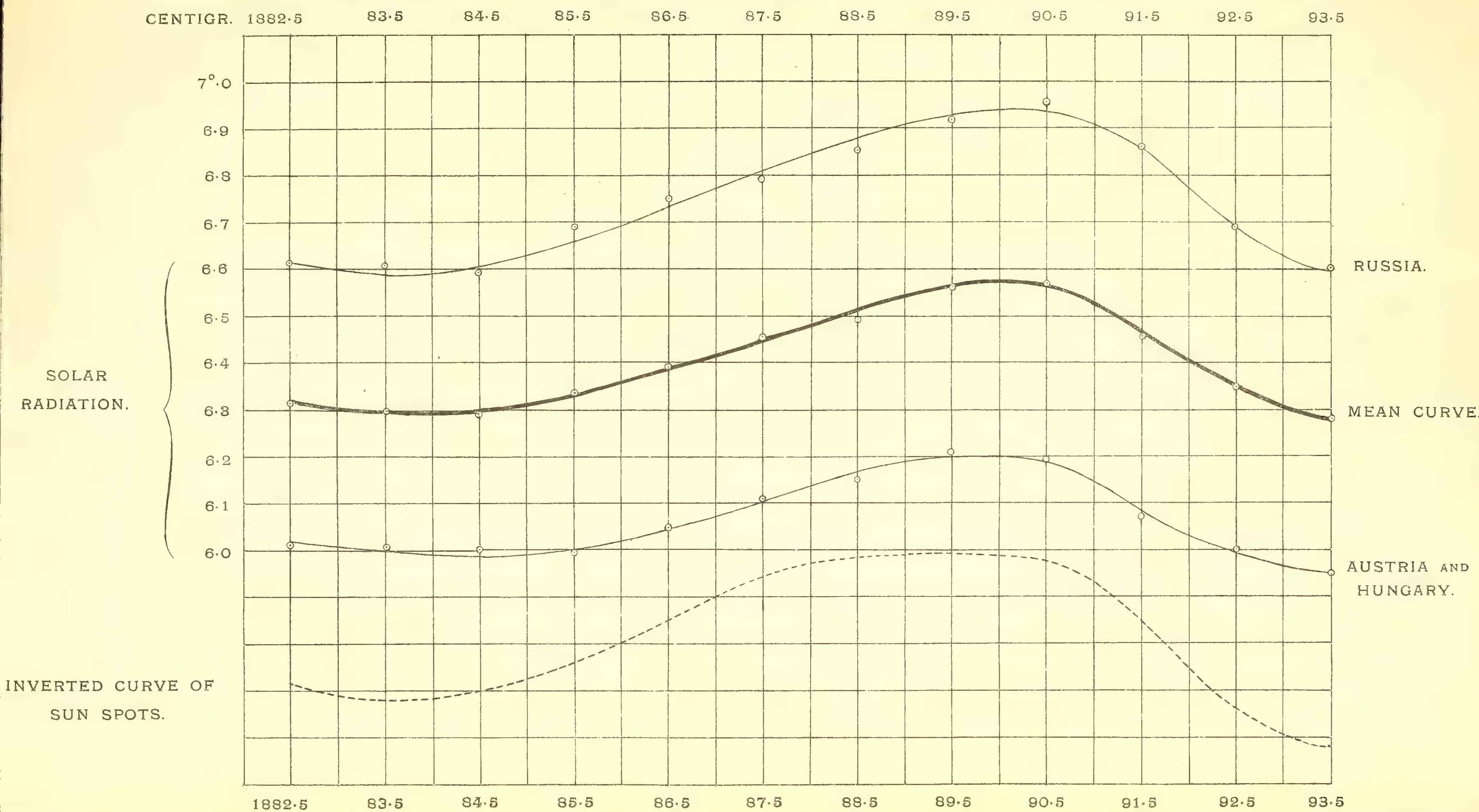


and for both countries. For the same interval, extending from 1882 to 1893, we have drawn the inverted curve of sun spots. The agreement between the two curves may be considered as perfect. There are a good many stations the observations of which extend over more than 100 years. They afford a large field for research, on the probability of secular variations of solar heat and other important questions in connection therewith.

It is necessary for me to conclude this paper with a mere mention of some other consequences of our theory, which contribute to the solution of the question, so important in practical meteorology, how to determine the corrections for reducing single observations to the true arithmetic mean of the day. I may say that these corrections can be derived directly from the above-mentioned tables for a, without referring to normal places, as was hitherto the case. Nor will I dwell any longer on other interesting particulars respecting the daily curve thus theoretically deduced, as I have given a somewhat full account of several of them in the paper already published.

I would desire to give expression to the hope that these researches, so imperfectly sketched in this paper, may induce meteorologists to undertake further investigations in this field of natural science.

The Intermediate Observing Station on Ben Nevis. By T. S. Muir, M.A.

(Read January 4, 1897.)

It has for some time been the design of the Council of the Scottish Meteorological Society to establish on Ben Nevis a third observing station, supplementary to those at the summit and at Fort-William; and in the autumn of last year this design was partially realised. A site was not acquired so easily as might have been expected; but, after some disappointment, the Council, on the advice of Dr Buchan, who in person inspected the ground, fixed upon the best situation obtainable in the circumstances. Before the end of August the observer was in residence, but as the barometer did not arrive till the last day of the month, the complete record of observations extends only from the 1st to the 23rd September—23 days in all. The total number of readings taken was 186, an average of slightly more than 8 every 24 hours. Six of those observations were made at the hours of 9, 10, 12, 14, 18, and 21, while additional readings ran in a regular series between those fixed periods; thus accumulating a mass of facts for all times of the day and in various kinds of weather. The gaps are accounted for by the fact that, combined with my meteorological duties, were the functions of cook and maid-of-all-work, which occasionally rendered it impossible for me to leave the base of operations. Further, it was set down in my instructions that it was unnecessary to go out during continued bad weather, except at the statutory periods above mentioned; and that, in the event of extra readings being required during storms, a message would be sent from head-quarters.

The instruments used were:

1. A Fortin barometer with extended scale, by Casella, London.
2. Dry and wet bulb, maximum and minimum thermometers.
3. Two 5-inch rain-gauges.

Clouds, wind, visibility, and fog were estimated independently of instrumental aid.

The barometer was set up in the hut as far from the cooking-stove as possible, and the thermometers (in Stevenson screen) and the rain-gauge were thirty yards away on the slope above.

The hut itself is on the summit of a slight ridge on the general slope of the hill, which slants on the south to a narrow cleft—the path of a torrent in its drop to the glen beneath—and on the north to a broader and deeper wedge running right into the heart of the mountain. Above is the Ben itself, or rather the shoulder of it called Cairn Dearg. To the north-west the view is partially blocked by a hill, which overlooks the hut from a distance of about half-a-mile and a height of 150 feet; and due west is the steep descent of the hill to the valley of the Nevis. The station has thus an open exposure to the north, the west, and the south-west. Its height above sea-level is 2190 feet.

The advantages of the site are—(1) It is almost exactly half-way in height and horizontal distance between the Summit and Low-Level Observatories, thus giving an indication as to whether the gradient of temperature is uniform.

(2) The slight ridge above mentioned, on each side of which there is clear space for the uninterrupted descent of cold down-flowing atmospheric currents.

(3) Being on a hill side, it possesses at least half the advantages of a summit station.

On the other hand, there are the following disadvantages :—

(1) A steep slope above, completely obstructing the eastern horizon.

(2) The liability of the barometer to the vibration and sudden changes of temperature inseparable from the construction of a wooden hut.

(3) The long continuance of cyclonic weather, that to some extent defeated the main purpose of the undertaking. All of those defects, however, except the last, are of slight importance compared with the advantages; and I venture to say that, in spite of the adverse weather conditions, results have been obtained such as will, if no more, justify another trial of the intermediate station.

The method adopted in the preparation of this paper was, first, to construct a number of tables containing the actual observations; and, secondly, to compare the simultaneous readings at the three stations.

PRESSURE, TABLE I.—Barometric pressure was highest, 27·784 inches, at 21 h. on the 6th, and lowest, 26·617 inches, at 18 h. on the 22nd, the wind being calm on both occasions. The range over the 23 days was thus 1·167 inch. The maximum occurred in the midst of a period of anti-cyclonic weather, which was pretty general over north-western Europe, but the dry weather of which was of longer continuance at the middle station than over the rest of the British Isles. With the exception of a slight shower on the 3rd, no rain fell from that day till the early morning of the 10th, and moderate to light winds were the rule till the 9th. The minimum occurred during a period of low-pressure general over the British Isles. The wind was squally from the east in the morning, but it died away later. During the following night there was a very heavy rainfall, amounting to nearly an inch in twelve hours, which, however, seemed to be confined to the upper half of the mountain.

TABLE II.—I have, by means of the "Challenger" tables, reduced each barometer reading to sea-level. A study of the differences between the barometers at the three stations shows that they are extremely variable. For example, one would expect that when the summit barometer read higher than the base barometer, that at the middle would read a little less higher, and, similarly, in the case of reading lower. The truth is, that more frequently than anything else the middle barometer read lower or higher than either of the other two. It would be necessary to obtain more numerous data before attempting to propound a general law, but it may be noted that out of the 22 times that the mid barometer read higher than that at the base, 14 occurred close together during the first four days of the month, and were followed by a period of fine weather; while the remaining 8 were scattered over the latter part of the month, and were extremely minute in amount. Taking all the observations into account, we find that on the average, the mid barometer read 0·010 inch lower than that at Fort-William, after all necessary corrections had been made.

The greatest depression was ·041 inch lower at noon on the 17th, followed by a heavy rainfall, which was greatest at Fort-William and least at the summit. The greatest excess was ·023 inch higher at 18 h. on the 3d, followed by six days of fine weather.

TEMPERATURE, TABLE III.—The maximum temperature, 59°·5, occurred between noon and 16 h. on the 13th, and the minimum, 35°·0, between 21 h. on the 20th and 9 h. on the 21st. At the latter hour the thermometer stood at 36·9. The range at the intermediate station was thus 24·5. The 13th was an exceedingly warm day, with bright sunshine, the maximum for the month being also recorded at Fort-William. On the night of the 20th–21st, temperature ruled low at all three places, the minimum being observed both at the summit and Fort-William. Thunder and lightning occurred on the 14th, while the 21st was cold, but fine and rainless.

Subjoined is a table giving the mean differences of temperature for the six standard hours.

S = Summit, I = Intermediate, B = Base. The first column gives the mean September differences for three years; the others for September 1–23, 1896.

	S. and B.	S. and B.	S. and I.	I. and B.
9	15·4	15·3	7·2	8·1
10	15·8	16·4	7·7	8·7
12	16·5	18·3	8·7	9·6
14	16·8	18·8	9·5	9·3
18	16·4	17·2	8·8	8·4
21	14·6	15·9	8·3	7·6
Mean, .	15·9	17·0	8·4	8·6

From it we find that from 9 to 12 in the morning the intermediate temperature came nearer to that of the summit, and from noon till 21 h. it was closer to that at Fort-William; while the mean day difference was as nearly as possible half of that between the summit and the base. It is probable, however, that during the night it is nearer to the summit temperature, and that the

average temperature for the 24 hours is lower than the mean of the summit and Fort-William. As might be expected, the differences rise to a maximum about mid-day, and fall towards evening. That with the summit occurred at 2 p.m., and that with the base at 12 noon.

As formerly mentioned, the slope of the hill entirely obstructs the eastern horizon, thus shading the intermediate station from solar action in the morning. This, coupled with the fact that the summit is open to such action, probably explains the relative closeness of the intermediate and summit temperatures in the early part of the day.

Table IV., at the end of this paper, gives the hour to hour differences in accessible form. As the maximum and minimum thermometers were read four times a day, it is possible, where no actual observation was made, to estimate the temperature at any given hour of the day with some approximation to accuracy. Such interpolations are marked with brackets.

On 14 days out of the 23, the difference from the summit was relatively small in the morning and larger at night; on 6 it was large in the morning and smaller at night; on 2 it was nearly the same; and on 1, the fifth, it was very small in both cases.

There was one occasion, on the morning of the 6th, when the temperature at the top of the mountain was higher than that at the middle; but unfortunately, owing to an accident, no observation was taken at the intermediate station before noon. According to the maximum thermometer, however, it could not have been more than 48°·1, and was probably much less, while the readings at the summit for the hours of 9 and 10 were 46°·0 and 48°·7, dropping within the next hour to 43°·2.

The temperature at the middle station was closest to that of the summit—(1) At 9 h. on the 1st of the month, when it was 1°·4 higher. The wind was N.E., light. There was sunshine, and the station was between two fog-systems—that is, fog above and below, with a clear space between.

(2) At 9 h. on the 2nd it was 2°·9 higher; wind N.E., light, and mist enveloping station, with occasional breaks.

(3) At 9 h. on the 5th it was 2°·8 higher; wind N., moderate in force, and station between fog-systems in sunshine.

(4) At 21 h. on the same day it was 1°·7 higher; wind E., very light, and station between two fog-systems.

On each of these occasions the difference from the base was considerably larger, though slightly below the mean. It will be noticed, that at each of those periods except the second, the station was in a kind of pond between two systems of fog, one of which belonged to the upper part of the mountain, with a *lower* surface varying from 2300 to 2600 feet, and the other to the lower part of the mountain, with an *upper* surface varying from 2000 to 2350 feet. In the second case there was mist at the station, and extending downwards, probably 400 feet.

The temperature at the middle station was closest to that of the base—(1) At 21 h. on the 2nd, when it was 3°·1 lower; wind N.N.E., light, and a small passing cloud on Meall-an't-Suidhe—the hill overlooking the station—between the heights of 2000 and 3000 feet.

(2) At 21 h. on the 10th it was 3°·1 lower; wind S., calm; no fog visible, and very clear.

(3) At 10 h. on the 21st it was 3°·9 lower; wind E.S.E., calm; no fog visible; bright sunshine at the station. During the night the minimum temperature was registered, and my thermometer gave a sudden leap from 36°·9 to 40·°9, while the base temperature rose only 2°·9. Afterwards there was a break away, and at 15 h. the difference was as much as 10°·7.

In the first of those cases there was only light passing fog about the level of the station, but half-a-mile away; and in the other two there was no fog at all.

It is worthy of notice that, while the summit was once warmer and twice within 2° of the intermediate, the latter was never warmer than the base, and never less than 3° colder at any observation.

The greatest differences from the summit temperature occurred:

(1) At 14 h. on the 13th when it was 13°·7 higher. The wind was W., light; there was bright sunshine, no fog visible, and it was the warmest day of the month.

(2) At 18 h. on the 14th it was 11°·9 higher. The wind was S.S.W., moderate to fresh—squally. There was fog about 600 feet above, and passing at the lake about the level of the station, and thunder later in the evening.

(3) At 15 h. on the 20th it was 13°·6 higher; wind, N.W., very light; bright sunshine; fog 1000 feet above.

(4) At 14 h. on the 21st it was 12°·6 higher; wind W., very light; bright sunshine; no fog visible.

In three of these cases there was either no fog visible, or it was at a considerable height; while in one case there was merely a passing cloud, not affecting the station itself.

The greatest differences from Fort-William temperature were—

(1) At 10 h. on the 11th it was 12°·5 lower; wind calm; no fog at the hour, but passing clouds.

(2) At noon on the 16th it was 12°·6 lower; wind W.S.W., squally; no fog visible; passing showers.

(3) At 15 h. on the 19th it was 12°·2 lower; wind S.S.W., moderate; fog about 100 feet above.

(4) At 15 h. on the 23rd it was 12°·7 lower; wind N.N.W., gusty; small rain falling; fog 200 feet above.

The difference from summit temperature was also considerable. Those facts seem to point to the following general conclusion:—That when the station was enveloped in fog or between two fog systems, or close to the fog, the temperature approximated to that of the summit, and when there was no fog visible, or if it were at a great height, it approximated to that of the base.

TABLE III.—As regards wet-bulb readings, the question of humidity is still so surrounded with difficulties that it is scarcely safe to enter upon it on such slight grounds. The air was dryest on the 13th of the month, the minimum humidity occurring at 14 h. The mean results show that humidity was greatest at 9 h., declined to a minimum at 14 h., and rose again towards evening. The amount of cloud and fog was greater in the morning than in the evening, which is the normal state of things.

TABLE IX.—This seems to be a fitting opportunity to enter more fully into a description of the fog. The height of its lower or upper surface was estimated by the help of well-known marks, the principal of which was the elevation of the station itself—2190 feet. In no case would the error exceed 100 feet, and any height near that of the station may be assumed to be fairly accurate.

On seven days well spread over the twenty-three, Ben Nevis, so far as visible from the station, was either all day or part of the day clear of fog. (I may remark that I could see to within 500 or 600 feet of the summit.) There was no day, however, when fog was not observed at some hour or other, but the 13th and 21st were almost wholly clear.

It frequently happened that the station was like an island surrounded by a sea of fog, which tried in vain to envelop it. The area thus unaffected was about 500 or 600 yards long and 100 to 200 yards broad. This is quite a distinct case from thin fog. It seemed as if the station were between two fog-systems, for frequently a view could be obtained betwixt them to the island of Lismore, thirty-two miles distant. The cause of this phenomenon is uncertain. The wind was always northerly when it occurred. Perhaps the shoulder of the mountain served to give the fog an upward and oblique slant, which carried on to Meall-an't-Suidhe, thus leaving the station clear. There were eight days when this was well defined. Once or twice it happened that fog coming down Glen Nevis, below the level of the station and apparently from the south, was met at the lake by the north wind and carried backwards and upwards, or dissipated (1st and 2nd September). In another case, when the wind was variable and light, fog coming from the north, between 2000 and 3000 feet, was met by west wind at the head of the corrie below and dissipated. It was very curious to see a cloud rolling along up to a certain point only to vanish into thin air.

Table V.—The amount of cloud, as distinct from fog or mist, was large. Taking a hundred to represent an overcast sky, the mean amount at 9 h. was 85; at 10 h., 81; at 12 h., 85; at 14 h., 88; at 16 h., 80; at 18 h., 86; and at 21 h., 75. The maximum was at 14 h. and the minimum at 21 h. It happened frequently that a starry night succeeded a cloudy day. On each of the three occasions when the intermediate temperature approximated to that of Fort-William, the sky was either wholly clear, as on the night of the 10th, or almost cloudless. On the other hand, when the middle temperature was very close to that of the summit, the sky was overcast, or nearly so.

Amount of Cloud at the Three Stations.

	9	10	12	14	16	18	21
BNO, .	8·4	8·0	8·8	9·7	9·6	9·4	8·6
IO, .	8·5	8·1	8·5	8·8	8·0	8·6	7·5
FWO, .	8·2	8·1	8·0	8·1	...	...	7·4

A comparison of the three stations shows that at 9 and 10 A.M. there was more cloud at the middle than at the top; while for the rest of the day there was less, and that all over there was more than at the base.

Table VI.—The station was not a favourable one for observing winds, as the entire eastern horizon was blocked up by the upper half of the mountain. Consequently, the only winds about whose true direction one could be certain were those from the north, the west, and the south-west. Northerly winds prevailed on eight days, chiefly at the beginning of the month, westerly on two, south-westerly on four, southerly on four, easterly on two, and light variable airs on three days. The strongest wind blew from the south-west on the night of the 14th, at a rate exceeding sixty miles per hour. It lasted for two days at slightly reduced speed, and brought heavy showers of rain.

Mean Force of Wind.—Scale, 0–12.

	9	10	12	14	18	21
BNO, .	1·96	2·13	2·11	2·32	2·13	2·20
IO, . .	1·22	1·20	1·41	1·61	1·17	1·41

The highest mean wind-pressure occurred at 14 h., being about 12 miles an hour, and the minimum at 18 h., 8 miles an hour. At the summit the highest was also at 14 h., 15–16 miles an hour, and the lowest at 9 h., 12–13 miles an hour. But, as I have already said, the station is not a suitable one for observations of

this nature; and, besides, the period was so short, that of wind-pressure, at least, little can be said.

TABLE VII.—*Total Rainfall.*

BNO, . .	=6·530 inches.	BN<IO . .	= ·592 inches.
IO, . .	=5·938 ,,	IO <FW, . .	=1·708 ,,
FWO, . .	=4·230 ,,	Bn<FW, . .	=2·300 ,,

So that, while the total precipitation at the summit was only three-fifths of an inch more than at the middle, the latter exceeded that at Fort-William by nearly $1\frac{3}{4}$ inches.

TABLE VIII. AGGREGATES FOR 12 HOURS.—Thrice rain fell at the summit and not at the other places, but the amount was inconsiderable.

Six times there was rain at the top and the middle, but none at the base.

On one of those occasions the amount was the same at both places; on the remainder the amount was greater at the summit, showing that the rain was confined to the upper half of the mountain.

Seven times the precipitation for 12 hours was greater at Fort-William than at the summit; at six of those the middle station was actually intermediate, and on the seventh no rain fell at all.

From 9 h. on the 12th to 9 h. on the 13th the rainfall at the middle station was greater by about $\frac{1}{5}$ of an inch and $\frac{3}{10}$ of an inch respectively than that at the summit and Fort-William. A glance at the Table shows that the barometer was lower than at either of the other two places, while the summit barometer read higher than that at the base, and the intermediate temperate was more irregular than at the other stations.

From 9 h. to 21 h. on the 16th the rainfall at the middle was less than at the other two places; but the wind was gusty.

Lastly, during the night of the 22nd there was a very heavy rainfall on the upper half of the mountain—$1\frac{1}{10}$ inch at the top and nearly 1 inch at the middle, but only a little over $\frac{1}{10}$ of an inch at Fort-William. From Table II. it will be seen that the sharp rise of the base barometer was not imitated by the other two, at least not to the same extent, and that the temperature at all three places rose towards evening. The wind at the top till

midnight blew at the rate of 40–50 miles an hour from the south-east, then changed to north, and became calm. The heaviest fall took place between midnight and 7 h.; on the other hand, at the middle station the wind was almost calm from 16 h. onwards. At 21 h. there was a light air from the north. At Fort-William the wind was also northerly.

The observations having been considered separately, it only remains to point in a word to any noteworthy connections as shown by the Tables. It is the rule to find any striking change of pressure or temperature in one case echoed in a lesser degree by the other two, but the apparent irregularities are numerous.

The sudden risings and fallings of temperature on the first two days of the month are probably due to the fact that on those days the station was visited by passing fog, punctuated by gleams of sunshine.

There was a wave of colder weather, lasting from the 5th to the 8th, broken only at the summit on the morning of the 6th. This was during the period of fine weather.

Thunder and lightning occurred on the 14th. The fall and sudden rise of the barometer was accompanied by a rise and equally sudden fall of the thermometer, which was most marked at the summit.

Lightning on the evening of the 16th was accompanied by a drop in the temperature at 16 h., followed by a temporary rise.

On the next day, the 17th, the barometer fell rapidly as temperature rose quickly, and the subsequent rise of the former was balanced by the fall of the latter. The wind at the middle station that day varied from 20 to 60 miles an hour; while at the top, from 14 to 19 h., it varied from 30 to 70 miles an hour.

My residence at the intermediate station was rendered comfortable and almost luxurious by the assiduous kindness and forethought of Mr Rankin, superintendent of the Observatory; and I am indebted to Professor Tait, Dr Buchan, and Mr Omond for many valuable hints in the preparation of this paper.

TABLE I.—20·000 + *Inches. Barometer (corrected to* 32°).

Hours..	9	10	11	12	13	14	15	16	17	18	19	20	21
Sept. 1,	7·703	7·709	7·710	7·713	7·716	7·705	7·695	7·689	7·678	7·678	7·680	7·673	7·680
,, 2,	7·589	7·591	7·577	7·578	..	7·556	..	7·544	7·536	7·530	7·537	..	7·525
,, 3,	7·544	7·550	..	7·531	7·529	7·530	..	(7·530)	..	7·530	..	..	7·519
,, 4,	7·577	7·590	..	7·608	..	7·606	..	7·601	..	7·603	..	..	7·615
,, 5,	7·665	7·664	..	7·663	..	7·673	..	7·675	..	7·678	..	..	7·697
,, 6,	(7·743	7·745)	..	7·751	..	7·764	..	7·767	..	7·771	..	..	7·784
,, 7,	7·776	7·774	..	7·779	..	7·777	..	7·764	..	7·751	..	..	7·753
,, 8,	7·676	7·666	..	7·666	..	7·653	..	7·630	..	7·602	..	..	7·604
,, 9,	7·462	7·451	..	7·431	..	7·427	..	7·395	..	7·400	..	..	7·363
,, 10,	7·284	7·294	..	7·318	..	7·340	..	7·360	..	7·389	..	..	7·400
,, 11,	7·406	7·410	..	7·399	..	7·387	..	7·376	..	7·372	..	..	7·368
,, 12,	7·333	7·318	7·306	7·302	7·293	7·266	7·229	7·220	..	7·201	..	..	7·171
,, 13,	6·990	6·981	6·977	6·964	..	6·952	..	(6·952)	..	6·952	..	..	6·964
,, 14,	6·900	6·891	..	6·854	..	6·796	..	(6·795)	..	6·795	..	..	(6·900)
,, 15,	7·160	7·164	..	7·206	..	7·204	..	(7·223)	..	7·242	..	..	7·270
,, 16,	7·213	7·246	..	7·255	..	7·269	..	(7·287)	..	7·305	..	..	7·356
,, 17,	7·374	7·350	..	7·258	..	7·104	..	(6·950)	..	6·798	..	..	6·982
,, 18,	7·171	7·177	..	7·146	..	7·119	..	(7·092)	..	7·065	..	..	7·072
,, 19,	7·100	7·109	..	7·125	..	7·141	7·128	7·142	7·151	7·177	..	..	7·196
,, 20,	7·334	7·345	7·352	7·375	..	7·380	7·371	7·379	7·394	7·404	7·409	7·407	7·415
,, 21,	7·349	7·343	7·343	7·340	7·329	7·312	7·295	7·265	7·256	7·253	7·243	7·222	7·192
,, 22,	6·805	6·763	..	6·724	..	6·675	..	6·663	..	6·617	..	..	6·639
,, 23,	6·975	7·001	7·046	7·077	7·110	7·150	7·172	7·204	7·234	7·256	7·272	7·276	7·297
Means,	7·353	7·354	..	7·350	...	7·338	..	7·326	..	7·320	..	..	7·337

TABLE II.—*Differences of Barometers at Sea-Level in Thousands of an Inch.*

First Column, Summit and Base; Second, Intermediate and Base.

Black type means summit or intermediate higher than base, and italic lower.

Hours.	9		10		12		14		16		18		21	
Sept. 1, .	*3*	*21*	*6*	*19*	*16*	*17*	**0**	*13*	**4**	*12*	**9**	**2**	**15**	*2*
,, 2, .	**31**	*11*	**8**	*8*	**9**	*3*	**13**	*8*	**15**	**4**	**29**	**2**	**28**	*9*
,, 3, .	**24**	**2**	**20**	*4*	**4**	**9**	**3**	*3*	**1**	(8)	**18**	**23**	**26**	**2**
,, 4, .	**23**	*7*	**9**	*3*	**7**	*3*	**20**	*1*	**14**	**3**	**29**	**2**	**30**	*3*
,, 5, .	**3**	*7*	*3*	*16*	*5*	*23*	**4**	*1*	**7**	*9*	*2*	*22*	*30*	*21*
,, 6, .	*25*	*(14)*	*36*	*(11)*	*22*	*16*	*24*	*12*	*7*	*5*	**5**	*4*	**10**	*9*
,, 7, .	*2*	*19*	*8*	*20*	**6**	*10*	*2*	*6*	**3**	*4*	**9**	*5*	**7**	*5*
,, 8, .	**20**	*12*	**18**	*17*	**1**	*9*	**9**	**0**	**2**	*12*	**17**	**0**	**16**	**4**
,, 9, .	*8*	*12*	*28*	*33*	*4*	*36*	*9*	*21*	*9*	*36*	*26*	*10*	*5*	*18*
,, 10, .	**3**	*3*	**5**	*10*	*10*	*12*	**7**	*16*	**13**	*2*	**19**	*4*	**43**	**0**
,, 11, .	*24*	*15*	*19*	*5*	*11*	*9*	*15*	*15*	*25*	*16*	*8*	*10*	*23*	*19*
,, 12, .	**7**	*5*	*13*	*10*	*9*	*12*	*19*	*19*	**4**	*24*	**11**	*13*	**16**	*9*
,, 13, .	**12**	*9*	**4**	*13*	**6**	*15*	**8**	*21*	**13**	*(7)*	**20**	*4*	**25**	**3**
,, 14, .	**9**	*10*	**1**	*8*	**4**	**5**	*11*	*9*	*16*	*(10)*	**5**	*12*	*10*	*(11)*
,, 15, .	*10*	*9*	*10*	*28*	*10*	*14*	*9*	*20*	**0**	(9)	**22**	*5*	**6**	*10*
,, 16, .	*24*	*32*	**4**	*9*	*3*	*10*	*21*	*25*	**11**	(6)	**4**	*4*	**9**	**7**
,, 17, .	**4**	*4*	*22*	*9*	*29*	*41*	*55*	*24*	*18*	*(20)*	*35*	*17*	*38*	**2**
,, 18, .	**0**	*6*	*6*	*3*	*10*	*12*	*15*	*14*	**2**	*(7)*	*7*	*8*	*5*	*19*
,, 19, .	*6*	*10*	**1**	*12*	*8*	*9*	*8*	*8*	*3*	*10*	*8*	**7**	**9**	*8*
,, 20, .	*10*	*20*	*9*	*19*	*13*	*11*	*4*	*13*	*4*	*18*	*2*	*14*	**2**	*12*
,, 21, .	**8**	*13*	**4**	*15*	*4*	*12*	*12*	*20*	**2**	*21*	**11**	*1*	**3**	*8*
,, 22, .	*33*	*27*	*45*	*37*	*23*	*26*	*2*	*7*	**13**	**1**	**22**	*5*	**3**	*25*
,, 23, .	*29*	*10*	*31*	*18*	*34*	*20*	*39*	*9*	*14*	*10*	*10*	**7**	*12*	*6*
Means, .	*1*	*12*	*7*	*14*	*8*	*13*	*8*	*12*	**0**	*10*	**6**	*4*	**5**	*8*

TABLE III.—*Thermometer (Dry and Wet Bulbs).*

Hours.	9		10		11		12		13		14		15		16		17		18		19		20		21			
	D.	W.	D.	W.	D.	W.	D.	W.	D.	W.	D.	W.	D.	W.	D.	W.	D.	W.	D.	W.	D.	W.	D.	W.	D.	W.	Max.	Min.
Sept. 1,	48·4	47·3	50·5	49·4	51·8	49·8	51·6	49·5	53·8	52·2	53·4	51·3	54·4	52·0	53·3	51·3	53·5	51·7	50·2	48·8	49·2	49·2	50·6	48·8	51·1	49·8	55·8	47·1
,, 2,	49·9	49·9	51·0	51·0	53·1	53·1	53·8	53·8	.	.	53·8	53·8	.	.	53·0	53·0	53·4	53·4	55·7	54·3	54·3	53·5	.	.	55·9	52·8	57·6	48·0
,, 3,	53·3	51·9	55·8	52·4	.	.	54·1	53·3	54·5	52·0	54·7	51·7	.	.	(53·0)	.	.	.	50·9	50·6	.	.	.	.	50·8	50·2	57·0	50·4
,, 4,	53·2	52·2	54·0	52·2	.	.	54·6	52·7	.	.	55·1	53·4	.	.	54·4	53·2	.	.	53·2	52·1	.	.	.	.	51·1	49·8	56·8	50·0
,, 5,	46·8	46·8	48·8	48·2	.	.	51·3	50·5	.	.	49·8	49·4	.	.	51·1	49·2	.	.	48·5	46·6	.	.	.	.	46·7	44·8	52·9	44·6
,, 6,	(45·5)	(45·0)	(46·0)	(45·2)	.	.	47·5	46·4	.	.	51·3	49·6	.	.	49·2	47·3	.	.	46·4	45·5	.	.	.	.	45·6	44·7	52·5	41·8
,, 7,	45·0	44·8	45·3	45·1	.	.	46·9	46·1	.	.	47·2	45·8	.	.	47·0	45·5	.	.	46·6	44·8	.	.	.	.	45·4	44·4	48·0	42·9
,, 8,	46·5	44·6	45·9	44·9	.	.	50·2	47·6	.	.	52·8	49·1	.	.	52·3	49·0	.	.	49·6	47·2	.	.	.	.	47·8	46·8	53·5	43·5
,, 9,	50·4	48·4	50·8	46·3	.	.	52·1	46·6	.	.	51·6	46·1	.	.	51·1	46·1	.	.	51·4	49·2	.	.	.	.	50·4	49·3	56·3	46·0
,, 10,	52·4	50·1	53·2	52·4	.	.	53·7	51·5	.	.	54·8	51·7	.	.	55·4	52·1	.	.	54·3	51·2	.	.	.	.	52·4	49·0	56·7	48·7
,, 11,	50·8	48·7	52·8	49·2	.	.	53·3	51·2	.	.	55·0	52·5	.	.	53·4	52·3	.	.	53·9	52·3	.	.	.	.	52·8	50·3	56·2	50·3
,, 12,	50·4	50·1	54·3	49·8	53·3	48·8	54·4	50·1	54·2	49·5	54·1	49·8	52·0	48·5	51·3	47·6	.	.	49·9	47·9	.	.	.	.	50·2	47·9	57·1	48·5
,, 13,	49·4	46·4	51·8	47·0	51·7	48·6	52·9	48·4	.	.	57·6	51·2	.	.	(56·5)	.	.	.	55·0	50·0	.	.	.	.	51·1	48·3	**59·5**	47·0
,, 14,	47·8	47·2	48·5	47·3	.	.	48·4	47·6	.	.	47·6	45·5	.	.	(48·0)	.	.	.	48·5	43·3	.	.	.	.	(45·5	45·5)	52·7	43·1
,, 15,	41·4	39·9	42·3	41·0	.	.	39·9	39·5	.	.	44·5	42·7	.	.	(43·8)	.	.	.	42·9	42·4	.	.	.	.	42·1	41·3	45·2	37·9
,, 16,	42·2	41·3	42·3	42·1	.	.	42·9	41·3	.	.	43·1	40·7	.	.	(43·0)	.	.	.	42·9	40·9	.	.	.	.	41·6	40·1	44·0	38·0
,, 17,	43·5	43·3	43·5	43·1	.	.	44·3	43·1	.	.	46·0	43·8	.	.	(50·0)	.	.	.	53·7	51·6	.	.	.	.	42·3	41·5	55·0	40·5
,, 18,	41·6	40·9	42·2	41·4	.	.	43·0	42·8	.	.	42·3	41·9	.	.	(41·0)	.	.	.	39·0	38·4	.	.	.	.	38·9	38·6	44·6	37·9
,, 19,	38·7	38·7	39·4	39·4	.	.	39·2	39·2	.	.	40·9	40·1	41·4	40·4	39·5	39·4	39·5	39·3	39·3	38·9	.	.	.	.	38·9	38·4	44·0	37·4
,, 20,	40·1	38·7	40·3	38·6	41·0	40·1	42·2	40·2	.	.	43·8	41·4	46·2	43·8	43·9	41·4	41·6	40·0	41·6	39·9	40·4	38·8	39·9	38·4	38·9	37·3	46·2	37·7
,, 21,	36·9	34·9	40·9	38·4	40·9	39·2	42·2	40·3	44·7	42·2	46·6	42·5	45·0	41·7	45·1	42·0	44·0	40·2	43·6	39·9	43·2	40·4	42·9	40·4	42·6	39·5	47·8	**35·0**
,, 22,	42·9	41·3	42·2	41·1	.	.	42·7	41·8	.	.	44·0	43·8	.	.	45·6	44·6	.	.	44·9	43·8	.	.	.	.	44·6	44·1	45·7	41·1
,, 23,	41·9	41·7	42·0	42·1	42·6	42·1	42·1	41·2	42·6	41·0	42·1	40·3	41·1	40·3	40·7	39·1	40·3	38·7	39·9	39·0	39·7	39·1	39·3	38·6	38·8	38·3	45·1	38·5
Means,	46·0	45·0	47·1	45·5	.	.	48·0	46·3	.	.	49·2	46·9	.	.	48·8	.	.	.	47·9	46·0	.	.	.	.	46·3	44·9	51·9	43·3

TABLE IV.—*Differences of Temperature.*

First Column, Summit and Base ; Second Column, Intermediate and Base.

Hours.	9		10		12		14		16		18		21	
	°	°	°	°	°	°	°	°	°	°	°	°	°	°
Sept. 1, .	7·0	5·6	8·6	5·0	11·8	7·7	17·4	9·0	18·1	10·6	17·5	11·3	14·6	7·0
,, 2, .	10·0	7·1	9·9	6·2	10·6	4·6	12·3	6·2	11·6	7·2	12·4	4·3	12·2	3·1
,, 3, .	14·9	6·5	17·8	6·8	17·6	8·5	19·8	9·7	17·7	(9·0)	14·6	7·9	13·6	6·7
,, 4, .	15·9	6·7	16·8	7·4	19·3	9·6	19·3	8·9	17·1	9·4	13·1	5·0	14·4	4·2
,, 5, .	11·5	8·7	12·6	8·8	17·5	9·8	17·0	10·2	16·5	8·1	16·0	8·9	9·0	7·3
,, 6, .	7·9	(8·4	6·3	(9·0)	13·3	9·4	12·2	7·2	14·7	8·5	14·6	9·2	12·6	7·0
,, 7, .	15·0	8·0	15 6	8 9	17·1	9·2	17·4	10·1	15·9	8·6	15·6	8·0	16·4	8·6
,, 8, .	14·5	5·8	15·4	7·5	17·7	6·9	20·1	7·6	21·7	9·4	18·2	8·5	16·5	8·4
,, 9, .	14·9	4·5	21·2	10·2	24·1	11·5	23·3	10·4	18·8	7·2	17·1	7·4	15·6	7·8
,, 10, .	15·3	6·1	16·1	7·4	16·4	7·0	16·3	6·2	17·5	7·2	17·2	7·2	13·0	3·1
,, 11, .	20·9	12·3	22·8	12·5	20·7	11·3	19·4	9·3	19·2	10·6	18·3	9·3	18·2	9·5
,, 12, .	16·8	9·2	18·2	7·6	22·5	11·2	22·2	10·9	20·2	10·9	20·3	10·3	15·9	6·7
,, 13, .	18·0	8·6	19·0	8·0	21·1	10·4	21·0	7·4	21·5	(9·0)	21·1	8·0	18·0	7·9
,, 14, .	12·5	5·1	12·7	4·9	16·3	8·0	17·0	8·4	14·8	(6·8)	17·4	5·5	20·2	(8·2)
,, 15, .	19·6	10·2	20·1	10·7	18·0	10·1	21·3	11·4	18·1	(9·3)	15·6	7·1	17·7	8·9
,, 16, .	17·5	9·3	18·7	10·7	22·3	12·7	21·9	11·8	18·0	(7·0)	19·8	10·5	18·6	10·3
,, 17, .	15·9	8·0	17·3	9·5	18·0	9·7	18·1	8·8	11·1	(3·9)	18·4	9·3	19·1	11·4
,, 18, .	20·1	10·7	20·2	11·1	19·1	11·2	17·0	8·7	16·5	(8·0)	17·4	10·0	17·3	9·5
,, 19, .	18·3	10·6	18·7	10·5	18·3	10·8	20·3	11·9	17·3	9·5	18·1	10·0	15·2	7·1
,, 20, .	17·4	8·5	19·2	10·7	20·9	11·2	22·3	11·2	21·9	10·1	18·3	8·4	15·2	8·0
,, 21, .	11·0	5·0	13·1	3·9	18·2	7·7	20·3	7·7	22·0	10·1	18·1	7·1	16·3	6·1
,, 22, .	20·6	9·9	19·9	10·8	18·5	9·9	16·6	8·5	17·1	7·2	16·6	8·8	15·5	7·7
,, 23, .	18·7	11·1	18·4	10·7	20·9	12·6	21·1	12·0	20·8	11·7	20·3	11·5	19·7	11·7
Means, .	15·4	8·1	16·4	8·6	18·3	9·6	18·8	9·3	17·7	8·7	17·2	8·4	15·9	7·6

TABLE V.—*Amount of Cloud* (0–10).

Hours.	9	10	11	12	13	14	15	16	17	18	19	20	21
Sept. 1, .	4	8	8	9	9	9	9	9	8	9	10	10	8
,, 2, .	10	10	10	10	.	10	10	10	7	8	5	.	5
,, 3, .	8	8	.	9	9	9	.	(8)	.	7	.	.	8
,, 4, .	7	8	.	8	.	8	.	8	.	7	.	.	2
,, 5, .	10	8	.	10	.	10	.	6	.	7	.	.	3
,, 6, .	9	9	.	9	.	4	.	8	.	8	.	.	8
,, 7, .	10	10	.	10	.	10	.	10	.	10	.	.	10
,, 8, .	10	10	.	8	.	10	.	10	.	10	.	.	10
,, 9, .	9	8	.	4	.	10	.	9	.	10	.	.	10
,, 10, .	8	9	.	8	.	8	.	8	.	8	.	.	0
,, 11, .	7	5	.	9	.	9	.	10	.	10	.	.	8
,, 12, .	8	3	4	6	7	7	9	9	.	10	.	.	9
,, 13, .	5	5	7	5	.	4	.	(5)	.	6	.	.	6
,, 14, .	10	10	.	10	.	10	.	(10)	.	10	.	.	8
,, 15, .	10	9	.	8	.	10	.	(10)	.	10	.	.	6
,, 16, .	9	9	.	8	.	8	.	(8)	.	8	.	.	8
,, 17, .	10	10	.	10	.	10	.	(10)	.	9	.	.	10
,, 18, .	9	10	.	10	.	10	.	(10)	.	10	.	.	10
,, 19, .	10	7	.	10	.	9	10	10	10	10	.	.	10
,, 20, .	9	5	8	9	.	8	8	6	9	8	10	4	4
,, 21, .	4	7	5	7	7	8	8	5	4	4	9	10	10
,, 22, .	10	10	.	10	.	10	.	9	.	10	.	.	10
,, 23, .	10	10	10	9	9	10	10	9	9	10	10	9	9
Means, .	8·52	8·09	.	8·52	.	8·78	.	8·56	.	8·65	.	.	7·48

TABLE VI.—*Direction and Force of Wind* (*Scale* 0–12).

Hours.	9	10	11	12	13	14	15	16	17	18	19	20	21	REMARKS.
	D F	D F	D F	D F	D F	D F	D F	D F	D F	D F	D F	D F	D F	
Sept. 1, .	N.E. 1	N.E. 1	N.E. 2	N.E. 1–2	N.E. 2	N. 1–2	N.N.E. 1–2	N.NE. 1–2	N.N.E. 1–2	N. 0–1	N. 1	N.E. 2	N.N.E. 2	Northerly.
,, 2, .	N.E. 1	N.E. 0	N.N.E. 0–1	S. 0		N.N.E. 0–1	.	N.N.E. 0–1	N. 0	var. 0	var. 0	.	N.N.E. 1	Light, northerly and variable.
,, 3, .	N. 2	N. 0–1	.	N.N.E. 1–2	N.N.E. 2–3	N.N.E. 2–3	.	.	.	N.N.E. 1–2	.	.	E. 0	Northerly, then light, variable.
,, 4, .	S.W. 0	S.S.W. 0	.	N.N.W. 0	.	S.W. 0	.	N.N.E. 1	.	E. 0	.	.	E. 0	Light, variable airs.
,, 5, .	N.N.E. 0–1	N.N.E. 1	.	N.N.E. 1–2	.	N.N.E. 1–2	.	N.N.E. 2–3	.	N.N.E. 2–3	N.N.E.	N.N.E.	N.N.E. 3	Northerly, increasing.
,, 6, .	N. 0–1	N. 0–1	.	N. 0–1	.	N. 1	.	N. 1	.	N. 0–1	.	.	N. 0	Northerly, light.
,, 7, .	N. 0	N. 0	.	S. 0	.	N. 0	.	N.N.E. 0–1	.	0 0	.	.	E. 0–1	Northerly, then variable.
,, 8, .	0 0	0 0	.	S. 0–1	.	S. 0–1	.	var. 0–1	.	0 0	.	.	S.S.E. 2–3	Light, variable, then southerly. increasing.
,, 9, .	S.E. 2–4	E. 3–4	.	E. 4–5	.	E. 4–5	.	E. 4	.	N. 1	.	.	N. 2–3	Easterly, squally, then northerly.
,, 10, .	S.S.E. 1–2	S.S.E. 0–1	.	S. 0–1	.	S.S.W. 1	.	S.S.W. 1–2	.	S. 1	.	.	S. 0	Southerly, moderate, then calm.
,, 11, .	N.N.E. 0	0 0	.	var. 0	.	W. 1–2	.	S.W. 1–2	.	W. 0–1	W.	W.	W. 1–3	Variable, then westerly, gusty.
,, 12, .	S.S.W. 0–1	S.S.E. 0	W. 0	S.W. 0–1	S.W. 0	W.S.W. 1	E. 2–3	E. 3	.	E. 3	.	.	E.S.E. 1–2	Southerly, then easterly, gusty
,, 13, .	S. 0	E.S.E. 0–1	S.S.E. 1–2	E.S.E. 2	.	W. 0	.	.	.	N. 0	.	.	0 0	South-easterly, then variable.
,, 14, .	S.S.W. 2	S.S.W. 2–3	.	S.W. 2	.	S.W. 2–3	.	.	.	S.S.W. 3–4	.	.	W. 6	South-westerly and westerly, strong.
,, 15, .	S. 4–5	S. 4	.	S. 3–4	.	S.S.W. 4	.	.	.	S.W. 2–3	.	.	S. 3	South-westerly, squally (7–8 during night).
,, 16, .	S.S.W. 3–4	W.S.W. 4	.	W.S.W. 3–4	.	S.W. 4	.	.	.	W. 3	W.	W.	W. 2–4	South-westerly and westerly, squally.
,, 17, .	S.S.W. 2–3	S.S.W. 2–3	.	S.W. 3	.	S.S.W. 3–4	.	.	.	S.S.W. 4–7	.	.	W .S.W.	Southerly, strong and squally.
,, 18, .	S.S.W. 2	S.S.W. 2–3	S.W.	S.W. 3–4	S.W.	S.W. 2–3	S.W.	S.W.	S.W.	S.W. 1–2	S.W.	S.	S. 0	South-westerly, squally, then calm.
,, 19, .	S.S.W. 1–2	S.S.W. 1–2	S.W.	S.S.W. 0–1	S.S.W.	S.S.W. 0–2	S.S.W.	S.S.W. 0–1	S.S.E. 0–1	S. 0	.	.	0 0	Southerly, moderate, then calm.
,, 20, .	N.N.E 1–2	N.N.E. 0–1	N.N.E. 0	N. 0	.	N. 0–1	N.W. 0–1	N. 0–1	N.N.E. 1–2	N. 0–1	N.N.E. 0–1	N. 0	N. 0–1	Northerly, moderate, then calm.
,, 21, .	E.S.E. 0	E.S.E. 0	0 0	0 0	W. & S. 0	W. 0–1	N. 0–1	N. 0–1	W. 0	S. 0	S.S.E. 0–1	0 0	E. 0–2	Variable, light, then easterly, moderate.
,, 22, .	E. 0–4	N. 0–4	.	E. 0–4	.	N.E. 0–3	.	N. 0–1	.	0 0	.	.	N. 0–1	Squally and gusty, then calm.
,, 23, .	N. 1–2	N. 0–1	N. 2	N. 1–2	N. 1–2	N.N.W. 1–2	N.N.W. 0–2	N. 0–1	N. 0–1	0 0	N. 0–1	0 0	S.W. 0–1	Northerly, moderate, then calm.
Means, .	1·22	1·20		1·41		1·61				1·17			1·41	The highest wind (7–8) was from S.S.W. on the night of the 15th.

Table VII.—*Rainfall (5-inch Gauge.)*

Blank means no Observation taken ; — means no Rainfall.

Hours.	9	10	11	12	13	14	15	16	17	18	19	20	21	Total.
Sept. 1,	—	—	—	—	—	—	—	—	—	—	—	—	—	—
,, 2,	·044	—	·005	—	—	—	—	·003	—	—	—	—	—	·052
,, 3,	—	—	—	—	—	—				·006	—	—	—	·006
,, 4,	—	—	—	—	—	—	—	—	—	—	—	—	—	—
,, 5,	—	—	—	—	—	—	—	—	—	—	—	—	—	—
,, 6,	—	—	—	—	—	—	—	—	—	—	—	—	—	—
,, 7,	—	—	—	—	—	—	—	—	—	—	—	—	—	—
,, 8,	—	—	—	—	—	—	—	—	—	—	—	—	—	—
,, 9,	—	—	—	—	—	—	—	—	—	—	—	—	—	—
,, 10,	·134			·019		·050							·005	·208
,, 11,	·013	—	—	—	—	—	—	—		·005	—	—	—	·018
,, 12,	·140	—	—	—	—	—	—	·044		·251			·011	·446
,, 13,	·120	—	—	—	—	—	—	—	—	—	—	—	—	·120
,, 14,	·052			·138		·085				·261				·536
,, 15,	·299	·030		·115		·100				·018			·060	·622
,, 16,	·300	·068		·027		·005				·078			·013	·491
,, 17,	·236	·091		·108		·098				·202			·024	·759
,, 18,	·322	—	—	—		·024				·400			·155	·901
,, 19,	·250			·019		·050		·101	·009	·040			·060	·529
,, 20,	·004		·001	·014		·027		·102	·005	·064		·003	—	·220
,, 21,	·003	—	—	—	—	—	—	—	—	—	—	—	—	·003
,, 22,	—	—	—	—		·030		·010		·005			·016	·061
,, 23,	·951	—	·003	—	—	—	—	·009	—	—	·003	—	—	·966
Sums,	2·868	·189	·009	·440		·469		·269	·014	1·330	·003	003	·344	5·938

Table VIII.—*Comparative Rainfall for the Twelve Hours ending 9 and 21 Hours.*

First Column, Summit and Base ; Second Column, Intermediate and Base.

Black type means summit or intermediate higher than base.
Italic type ,, ,, lower than base.

Hours.	9		21		Hours.	9		21		Hours.	9		21	
Sept. 1,	..	..	..	..	Sept. 9,	**·007**	..	*·004*	*·013*	Sept. 17,	**·190**	**·099**	*·073*	*·007*
,, 2,	*·025*	*·006*	**·024**	**·008**	,, 10,	**·171**	**·090**	**·065**	**·054**	,, 18,	**·175**	**·152**	*·333*	*·146*
,, 3,	..	..	**·006**	**·006**	,, 11,	**·027**	**·013**	**·009**	**·005**	,, 19,	**·206**	**·108**	**·105**	**·039**
,, 4,	**·007**	..	..	..	,, 12,	**·171**	**·140**	**·075**	**·213**	,, 20,	**·008**	**·004**	**·217**	**·211**
,, 5,	..	..	..	..	,, 13,	**·029**	**·056**	..	..	,, 21,	*·010*	*·007*	..	..
,, 6,	..	..	..	..	,, 14,	**·043**	**·032**	**·057**	**·044**	,, 22,	..	..	**·097**	**·061**
,, 7,	..	..	..	..	,, 15,	**·192**	**·072**	*·300*	*·210*	,, 23,	**·981**	**·821**	**·094**	**·009**
,, 8,	..	..	..	..	,, 16,	*·171*	*·130*	**·240**	*·032*					

TABLE IX.—*Fog: Height and Changes.*

Sept. 1.—Till mid-day fog on Ben Nevis down to 2500–2900 ft.; at Lake and on Meall-an't-Suidhe from 1800 to 2300 ft.; in Glen Nevis, from 1000–2000 ft., constantly changing. All clear till 16 hrs., then fog on Ben Nevis down to 3200 ft. at 16 h., 1700 ft. at 19 h.,3000 ft. at 21 h., and rising in Glen Nevis up to 2250 ft. Station clear all day except at 17 h., passing clouds, bright sunshine.

,, 2.—Till 17 h. 30 mist down to 1600–1800 ft.; occasional breaks. At 17.30 the wind changed from N. to W., and the mist disappeared. At 21 h. light fog on Meall-an't-Suidhe between 2200–2300 ft. At 18 h. and 19 h., the fog coming from N.E. was blown back by W. wind.

,, 3.—At 9 h. slight fog on Ben Nevis at 2400 ft; Station clear; at Lake from 1700–2350 ft., then passing fog from 1900–2500 ft.; Ben clear till 13 h., then fog all day down to 2400–3000 ft., clear at 21 h.; from 18 h. fog at Lake 1800–2350 ft.; and at 21 h. in Glen Nevis 1900–2100 ft. Cloudy in morning, almost clear in evening; occasional sunshine.

,, 4.—At 9 h. passing fog at Lake from 1800–2000 ft.; then to 18 h. on Ben Nevis down to 2600–2800 ft.; at 21 h., station and Ben Nevis clear; at 18 h. at Lake 1800–2350 ft. Cloudy all day, almost clear at 21 h.

,, 5.—Till 14 h., passing fog from 1800–2600 ft.; then fog on Ben Nevis down to 2900 ft.; from 16 h. fog on Meall-an't-Suidhe at 2300 ft.; and in Glen Nevis from 1600–2000 ft.; none visible at 21 h. Cloudy all day, clear at night.

,, 6.—Till 16 h. fog on Ben Nevis down to 2500–2800 ft., then all clear. Cloudy all day, bright intervals.

,, 7.—Fog all day on Ben Nevis down to 2300–3500 ft.; at 9 h. on Meall-an't-Suidhe 2100–2300 ft.; in Glen Nevis, 1000–1800 ft.; at 10 h. on Meall-an't-Suidhe, at 2200 ft. Cloudy all day.

,, 8.—No fog all day except on Ben Nevis at 14 h. to 3600 ft., at 18 and 21 h. to 3000 ft. Cloudy and hazy all day.

,, 9.—Clear all day except on Ben Nevis at 9 h. to 3600 ft., at 18 h. to 3200 ft., at 21 h. thunder-clouds about 2500 ft. Cloudy in morning and evening, almost clear at noon, hazy, squally.

,, 10.—Till noon fog on Ben Nevis to 2900–3100 ft., then all clear. Cloudy in morning, very clear at night.

,, 11.—Fog on Ben Nevis all day from 2500–3100 ft. Fairly clear in morning, then cloudy.

,, 12.—At 9 h. fog on Ben Nevis to 3100 ft.; in Glen Nevis 1600–2300 ft., then clear all day. Clear first, then cloudy.

,, 13.—No fog visible all day except at 10 h. on Ben Nevis to 3900 ft. Fairly clear sky all day; bright sunshine.

,, 14.—Fog on Ben Nevis all day down to 3300–3500–2800–2300; at 9 h. scud in Glen Nevis 1200–2200 ft., and at 12 h. and 14 h. passing from 1200–2200 ft.; thunder at night; cloudy all day.

,, 15.—Fog on Ben Nevis all day down to 2900–2700 ft.; and at 12 h. and 18 h. on Meall-an't-Suidhe to 2250 ft., clear at 21 h. Cloudy all day; fairly clear at 21 h.

,, 16.—Till noon fog on Ben Nevis, 2700–3200 ft.; 10 h. at Lake 1800–2300 ft., then all clear. Thunder all evening. Cloudy all day, clearing a little at 21 h.

,, 17.—Till noon fog on Ben Nevis to 2350–3000 ft., then all clear. Sky overcast all day.

,, 18.—Fog on Ben Nevis all day from 2100–2600 ft.; from 14 h. on Meall-an't-Suidhe from 2100–2350 ft.; sky overcast.

,, 19.—Mist at times to 1800 ft.; passing at Lake, 1900–2250 ft.; at 21 h. scud at 2800 ft.; cloudy, with occasional breaks.

,, 20.—Fog on Ben Nevis up to 18 h. down to 2900–3400 ft.; then clear; at 20 h. rising in valleys to 1500 ft.; at 21 h. scud from 500–1500 ft. Cloudy first, then fairly clear; gleams of sunshine.

,, 21.—No fog all day except on Ben Nevis at 13 h. 3100 ft.; hazy; light cirrus in morning, overcast at night.

,, 22.—Till 16 h. fog on Ben Nevis down to 1900–3100 ft.; then clear at 21 h. to 2600 ft.; at 14 h. and 16 h. at Lake, 1700–2350 ft.; in Glen Nevis up to 1500 ft.; at 21 hours fog all round except at Station. Sky overcast all day.

,, 23.—Fog on Ben Nevis all day down to 1700–3000 ft.; at 9 h. in Glen Nevis down to 2000 ft.; at 20 h. cloud caps on hills around. Overcast all day, gleams of sunshine; at 10 h. mist to 1700 ft.

On the Electrolysis of Potassium Ethyl-Sulphone-Acetate. By Prof. Crum Brown and Dr Herbert W. Bolam.

(Read February 15, 1897.)

Ethyl-sulphone-acetic acid, $C_2H_5SO_2 - CH_2 - COOH$, was prepared by Claesson's method.* Fourteen grams of the potassium salt were dissolved in 20 c.c. of water, and the solution electrolysed for 30 minutes under following conditions:—Volts, 12; ampères, 3·4; temperature, 30°–35° C.

Carbonic anhydride and hydrogen were given off, and a white solid substance separated. This was collected on a filter and washed with cold water. The product, when dry, weighed 1·7 gram. It was purified by recrystallisation from chloroform.

It forms needles, melting at 136°–137°.

Analysis gave the following results:—

Weight of substance, . . .	0·2172 gram.
CO_2, ...	0·2645 „
H_2O, ...	0·1277 „

	Found.	Calculated for $C_6H_{14}S_2O_4$.
C, . .	33·21 per cent.	33·64 per cent.
H, . .	6·53 „	6·54 „

Weight of substance, . . .	0·2198
$BaSO_4$, ...	0·4652

	Found.	Calculated for $C_6H_{14}S_2O_4$.
S, . .	29·07 per cent.	29·91 per cent.

The results of analysis and the fusing point leave no doubt as to the identity of the substance, so that the electrolysis takes place in the normal manner,

$$C_2H_5 - SO_2 - CH_2 - COOK,$$

giving

$$C_2H_5 - SO_2 - CH_2 - CH_2 - SO_2 - C_2H_5 .$$

* *Bul. de la Soc. Chimique de Paris,* xxiii. p. 444 (1875).

Note on an Analysis of Human Gastric Juice. By **W. R. Lang**, B.Sc., F.C.S., Chemical Laboratory, University of Glasgow. (*Communicated by* Prof. M'KENDRICK.)

(Read March 15, 1897.)

In the autumn of 1895 I received from Professor M'Ewen several samples of gastric juice marked "Chalmers' Gastrostomy." The samples were obtained under most favourable circumstances, being drawn off through an aperture made in the abdomen and stomach of the patient; the juice was almost pure, and nearly free from suspended matter. My analysis was mainly directed towards ascertaining whether or not free hydrochloric acid was a constituent of the fluid, and the following were the reactions I employed :—

(1) With methyl violet the sample gave a distinct blue coloration, showing the presence of a mineral acid. Supposing the acidity to be due to lactic acid, this reaction would not take place, as on trying a dilute solution of lactic acid with this reagent no blue colour was produced.

(2) On treating an alcoholic solution of phloroglucin and vanillin (1 grm. of the former and $\frac{1}{2}$ grm. of the latter made up with 50 c.c.'s rectified spirits) with the gastric juice and evaporating slowly on a water-bath, a red colour was produced. Dilute hydrochloric acid gave the same result; lactic acid none.

In the opinion of some authorities the presence of free hydrochloric acid is attributed to the decomposition on evaporation of the chlorides present in the gastric juice by free lactic acid. With a view to ascertain whether this could happen or not, I evaporated solutions of chloride of calcium and lactic acid with both of the above reagents with a negative result. I also tried solutions of chlorides of the alkalies of various strengths with lactic acid under similar conditions, but in no case could I detect the slightest trace of free hydrochloric acid. These experiments clearly showed, therefore, that no such decomposition of the chlorides as was suggested could account for the reactions given with the gastric juice and the above reagents; and, consequently, the presence of free hydrochloric acid in the samples examined is clearly proved.

On a New Form of Constant Volume Air Thermometer, which shows the Total Pressure directly, and may be graduated in degrees by Temperature. By J. R. Erskine Murray, Assistant-Professor of Electrical Engineering and Physics in the Heriot-Watt College, Edinburgh.

(Read March 15, 1897.)

§ 1. The thermometer described in the present communication is a constant volume one. Its advantages proceed mainly from a simple arrangement whereby the total pressure of the enclosed air, and hence its temperature, since these are proportional, is

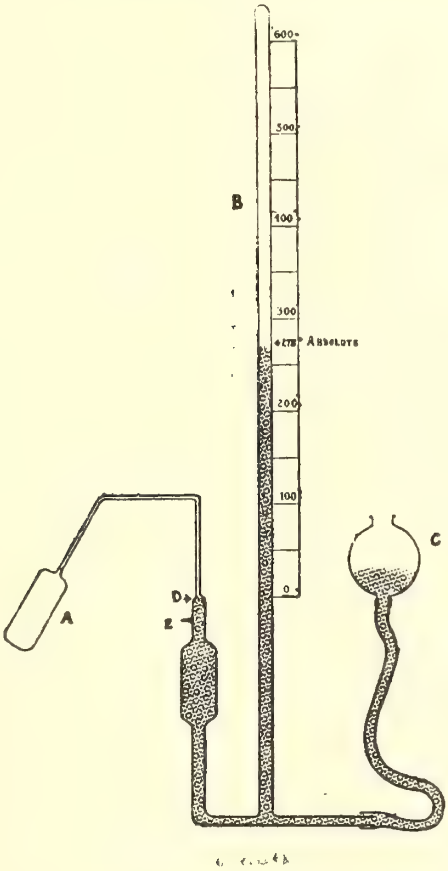


FIG 1

measured directly by the height of one column of mercury. The external atmospheric pressure is eliminated by the adjustment of an auxiliary reservoir of mercury.

§ 2. In fig. 1, A is the air-bulb, B is the pressure-gauge, which is an ordinary barometer tube with a vacuum above the mercury

connected at its lower end to the bent stem of the air-bulb and also by a flexible tube to the reservoir C. D is the constant volume mark on the stem of the air-bulb. The part of the instrument which is filled with mercury when the air is at 0° C. is shaded in the diagram.

§ 3. To make an observation of temperature the mercury is adjusted to the mark D by raising or lowering the reservoir. The height of the column in B above the level of the mark D now gives the total pressure of the enclosed air. The air-bulb and pressure-gauge may be cut off entirely from external pressure by closing the stop-cock between the pressure-gauge and the reservoir. If the absolute temperature corresponding to any given value of the pressure be known, we can calculate that corresponding to any other pressure by simple proportion.

Thus, in order to graduate the thermometer in degrees of temperature, it is only necessary to find the total pressure for any one temperature; that of melting ice, which we may take as 273° on the absolute scale of temperature, is the most convenient. To fix this point on the scale, the bulb is immersed in melting ice and the mercury adjusted to the volume mark by raising or lowering the reservoir. The height, h, of the mercury column in B above the level of the mark now corresponds to 273° absolute. But, since pressure and temperature are in simple proportion, the degrees at all parts of the scale are of equal length; thus $h/273$ is the length of one degree.

§ 4. It will be seen that the reservoir is merely a device for supplying mercury at the proper pressure to fill up the gauge-tube, and that it may be replaced by any suitable mechanical device, such as a cylinder full of mercury in which a piston is forced down by a screw. It is obvious that the height of the mercury in B above its level in the open reservoir C always corresponds to the atmospheric pressure at the moment. This portion of the apparatus is in fact a barometer if the reservoir C be open, but it is not in the least essential that it should be so.

§ 5. The atmospheric pressure is, after all, merely an *accident*; and its constant recurrence in all calculations on the pressure

of gases is very misleading to the student, who is apt to look upon it as a factor of a different nature to the observed pressure in the ordinary air thermometer, and hinders his realisation of the fact of the simple proportionality of temperature and pressure or volume.

§ 6. The length of a degree on this thermometer depends only on the total pressure at some given temperature; hence it is clear that the sensibility of the thermometer may be altered to suit any purpose by choosing a proper pressure at which to fill the bulb with air at the standard temperature. Thus, if great sensibility be required, the air when immersed in melting ice should be under great pressure, making the height h, which is equivalent to 273°, large; while, if a great range of temperature be required without great sensibility, it is only necessary to partially exhaust the bulb, at the same time placing the reservoir *below* the volume mark, so that the atmospheric pressure may be partially counterbalanced.

The length of the pressure-gauge tube of the thermometer may thus be indefinitely reduced, though of course at low pressures the open reservoir would require to be placed at nearly the barometric height below the volume mark.

The easiest way to obtain air at 0° C. at a pressure somewhat below that of the atmosphere is to fill the bulb with air at a high temperature, balancing the reduction of pressure and maintaining the volume constant as it cools by lowering the reservoir. When at 0° C. the air will now have a pressure below that of the atmosphere.

§ 7. In order to obviate the necessity for a correction for capillarity on account of the difference in the diameters of the pressure-gauge tube and the volume-gauge, the constant volume mark is put at the end of the fine tube of the stem, just where it joins the wider tube which forms its downward continuation. At this point the tube is conical, and its walls make an angle of about 45 degrees with the horizontal. Hence the mercury surface when at this point will be a plane, and its surface tension will act entirely in a horizontal direction.

§ 8. In order to be able to adjust the quantity of air in the bulb while filling it, a small capillary, E, is drawn out from the side of the stem at a point near the volume mark. This allows the air to escape as the mercury is introduced. The capillary is sealed before the reading of the pressure at 0° C. is taken.

§ 9. It would be possible to give the thermometer a greater apparent simplicity by omitting the auxiliary reservoir and making the pressure-tube movable, as in Joly's thermometer. The

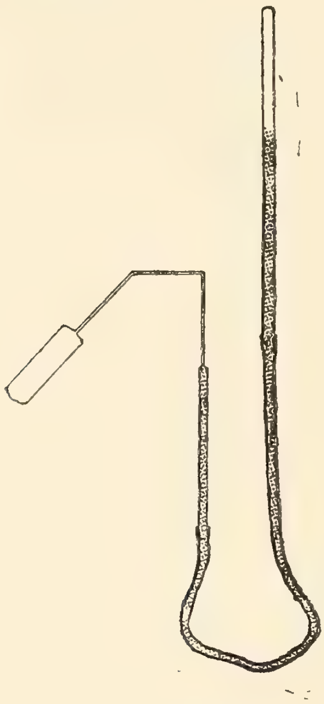


Fig. 2.

simplification is more apparent than real, however, and introduces several disadvantages. It does not do away with external pressure, as that will have its influence on the flexible connecting-tube. It would also be impossible to graduate the pressure-tube itself, and hence the objectionable arrangement of a scale on a mirror behind the tube would have to be used. The clamp which supported the main tube would also have to be in a more inconvenient position than that for the reservoir in the form in which I have described (see fig. 2).

On Photo-Chemical Action. By Professor John Gibson.

(Read February 15, 1897.)

A chief difficulty in the study of photo-chemical action lies in its variety, for it gives rise not merely to changes of a chemically simple kind, but also to the innumerable complex changes of plant-life and photography.

The difficulty is further increased by the extremely small proportion of material substance, chemically altered in a limited time by the action of those vibrations of the ether which we call light. To this, perhaps, more than to any other cause, must be attributed our continued inability to ascertain the true nature of photo-chemical changes so constantly observed as the decomposition of silver haloids by light.

Repeated attempts have been made to connect particular chemical effects, such as oxidation and reduction, with greater or smaller wave-length; but all such attempts have ended in self-contradiction (1, 2).

Now, if there be any single characteristic associating all photo-chemical actions together, this characteristic must be present in the simplest as well as in the most complex cases, and the consideration of the former seems, therefore, most likely to lead to definite conclusions.

Perhaps the most striking and the most simple case that presents itself is the increased electric conductivity imparted to crystalline selenium on exposure to light.

Monkmann has shown that pure soluble sulphur also acquires increased conductivity when exposed to light, though to a less degree than selenium (3).

Ultra-violet light (4), and also the Röntgen rays, impart electric conductivity to the air.

In these cases the change is a temporary one.

Ordinary phosphorus, under the action of light, is converted into red phosphorus—a substance possessing a markedly greater electric

conductivity. Similarly, red amorphous non-conducting selenium is gradually converted into the black crystalline conducting form, and red crystalline non-conducting mercuric sulphide changes to the black amorphous conducting modification (5, 6). These changes are permanent.

The common characteristic in these simple cases is thus an increase in the electric conductivity of the substances exposed to light.

Chemical Combination of Elementary Substances.

It may be doubted whether, strictly speaking, any instances of such photo-chemical action are known.

The classical example which most naturally suggests itself is that of a mixture of hydrogen and chlorine gases. It would appear, however, that the presence of water is essential to this action of light. It is certain that the gaseous mixture is much more sensitive to the action of light when moist.

Liquid hydrogen chloride, according to the researches of Grove, is not an electrolyte. On the other hand, in aqueous solution hydrogen chloride has a very high degree of electrolytic conductivity.

Reduction of Simple Metallic Compounds.

Here also it is matter for doubt whether, strictly speaking, any instances of photo-chemical action falling under this head are known. Nitrate of silver, as ordinarily prepared, blackens on exposure to light, but the pure nitrate of silver prepared by Stas remained perfectly white for years, although kept in an ordinary white glass bottle. That admixture with certain foreign substances greatly increases the sensitiveness of compounds of silver to light is a familiar phenomenon in photography.

The increase in electric conductivity of a film of silver haloid when exposed to light, is well illustrated in the actinometer devised by Arrhenius (7).

The oxides of gold and silver are stated to give off oxygen on prolonged exposure to light and to leave residues containing the highly-conducting metals (8, 9, 10). Similarly, mercurous oxide

is said to be decomposed by light into metallic mercury and mercuric oxide :

$$Hg_2O = HgO + Hg;$$

while mercurous chloride is split up into a mixture of metallic mercury and mercuric chloride.

Oxidation of Simple Metallic Compounds.

Lead oxide, under the influence of light, absorbs oxygen from the air, being ultimately converted into peroxide of lead (11).

Red lead (Pb_3O_4) and peroxide of lead (PbO_2) are both, relatively to lead oxide, good conductors of electricity, and with this conductivity their use in secondary batteries is connected.

Peroxide of manganese is another instance of a conducting metallic oxide. It is formed when manganous hydrate is exposed to air. Red light is said to accelerate and violet light to retard this oxidation (12). This latter statement is, however, admittedly problematical, and a consideration of the difficulty of arranging proper conditions for a crucial experiment makes it appear very doubtful.

Changes Produced in Aqueous Solutions.

Perhaps the most important example of this class of change is chlorine water. It is at best a very poor electrolyte, but the solution containing hydrochloric acid, which results from the action of light on chlorine water, has great electrolytic conductivity. It has been stated that this reaction is a reversible one, and it would be a matter of interest to determine experimentally whether this reversion can take place under conditions which involve a diminution of electrolytic conductivity.

Bromine water behaves similarly to chlorine water, while the very dilute solutions of iodine, which alone are obtainable, appear to be unaffected by light.

Sulphurous anhydride in aqueous solution, and in absence of oxygen, gives, on exposure to light, sulphuric acid and sulphur. Here also increase of electrolytic conductivity is obvious (13, 14).

Gaseous hydrogen iodide remains quite unchanged if kept in complete darkness, but decomposes on exposure to sunlight.

In absence of oxygen, aqueous solutions of hydrogen iodide are *not* decomposed by sunlight (15).

Potassium iodide, when dry, is not affected by light. Moist potassium iodide exposed to air and light is oxidised (16):

$$4KI + 2H_2O + O_2 = 4KOH + 2I_2 .$$

Here the solution of potassium hydrate is a better electrolytic conductor than the corresponding solution of potassium iodide.

Concentrated solutions of nitric acid are decomposed when exposed to the action of light, according to the equation:

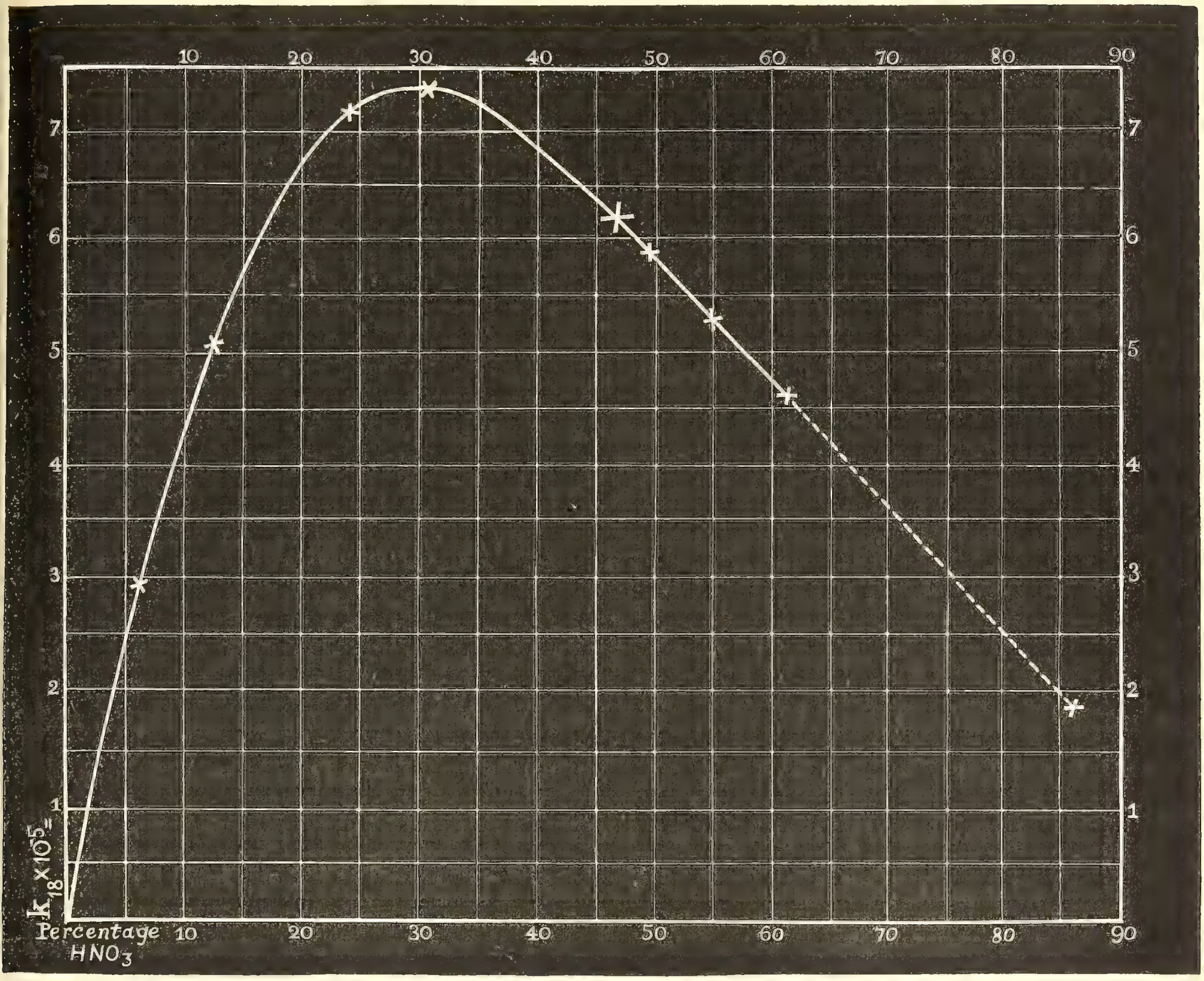
$$2HNO_3 = H_2O + N_2O_4 + O .$$

The acid acquires a yellow colour, owing to the dissolved nitrogen peroxide.

At first sight, this might seem an exception to the rule, as it involves the decomposition of an electrolyte without the formation of any better conductor, but it must be remembered that the electrolytic conductivity of nitric acid increases on dilution with water up to a certain maximum; on further dilution, the conductivity rapidly falls away. According to the determinations of Kohlrausch and Grotian (17), and E. Bouti (18), the maximum conductivity of nitric acid is reached when the solution has been diluted until it contains about 32 per cent. of nitric acid.

Now, nitric acid becomes less subject to the action of light as it is diluted, and if this action be related to increase of conductivity, we should expect to find that the action of light would cease to be readily observable at a dilution somewhat less than but not far from that corresponding to maximum conductivity. The most definite statement on this point which I have hitherto found is contained in Wurtz's *Dictionnaire de Chemie*; it is there stated that the action of light on the acid is the more rapid the greater the concentration, and that an acid having a specific gravity of 1·30 ceases to be coloured by light. Nitric acid of specific gravity 1·30 contains about 47 per cent. HNO_3; so that in this case also the rule holds, as will be readily seen by reference to the curve of conductivity of nitric acid.

Numerous other more or less complex cases might be quoted.



It will be sufficient for the present to give the following equations, taken from standard works on photography. To all familiar with recent theories of solution, the increase of electrolytic conductivity consequent on the action of light will be at once apparent.

$$2FeCl_3 + C_2H_6O = 2FeCl_2 + C_2H_4O + 2HCl$$
$$2FeCl_3 + H_2C_2O_4 = 2FeCl_2 + 2CO_2 + 2HCl$$
$$4HgCl_2 + 2H_2O = 4HgCl + O_2 + 4HCl$$
$$2HgCl_2 + (NH_4)_2C_2O_4 = 2HgCl + 2CO_2 + 2NH_4Cl \text{ (19).}$$

The examples cited so far seem sufficiently numerous and varied in character to justify the expectation that further investigation will prove the rule generally. This idea is further supported by a consideration of the means whereby we may increase or diminish sensitiveness to light in particular cases.

Photographically speaking, sensitizers are essentially halogen

absorbers, but it has to be observed that a part at least of the halogen thus absorbed goes to produce substances having a greater electrolytic conductivity than the sensitizers employed (*e.g.*, HCl).

On the other hand, the addition of substances which are good conductors diminishes the sensitiveness of the whole system to the action of light.

For instance, the action of light on chlorine water is greatly retarded by the addition of hydrochloric acid, potassium chloride, sodium chloride, &c.; the retardation being greatest in the case of hydrochloric acid, which is also the best electrolytic conductor (20).

It would be premature to suggest a theoretical explanation of the rule to which I have called attention. The fundamental conceptions cannot be better expressed than by the following quotation from Clerk Maxwell:—

"We know that the ether transmits transverse vibrations to very great distances without sensible loss of energy by dissipation. A molecular medium, moving under such conditions that a group of molecules *once* near together remain near each other during the whole motion, may be capable of transmitting vibrations without much dissipation of energy; but if the motion is such that the groups of molecules are not merely slightly altered in configuration, but entirely broken up, so that their component molecules pass into new types of grouping, then, in the passage from one type of grouping to another, the energy of regular vibrations will be frittered away into that of the *irregular* agitation which we call heat."

To this singularly lucid statement I would merely add the suggestion that, whenever molecules or groups of molecules are thus broken up, the new grouping *immediately consequent* upon the frittering away of the energy of regular vibrations will be itself always such as to oppose less resistance to the passage of an electric current than before. If the vibrations of the ether which we call light be indeed electro-magnetic, this result seems to follow as a natural consequence.

It is necessary, however, to distinguish between primary and secondary changes. It does not follow that increased conductivity necessarily implies chemical stability. The new grouping brought about by the action of light, while offering, so long as it

persists, a diminished resistance to the passage of an electric current, may be chemically unstable and tend towards a further rearrangement, offering an even greater electric resistance than that of the original molecular grouping.

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On the Linear and Vector Function. By Prof. Tait.

(*Abstract.*)

(Read March 1, 1897.)

In a paper read to the Society in May last, I treated specially the case in which the Hamiltonian cubic has all its roots real. In that paper I employed little beyond the well-known methods of Hamilton, but some of the results obtained seemed to indicate a novel and useful classification of the various forms of the Linear and Vector Function. This is the main object of the present communication.

1. It is known that we may always write

$$\phi\rho = \Sigma(\alpha S\alpha_1\rho)$$

and that three terms of the sum on the right are sufficient, and in general more than is required, to express any linear and vector function. In fact, all necessary generality is secured by fixing, once for all, the values of α, β, γ, or of α_1, β_1, γ_1, leaving the others arbitrary :—subject only to the condition that neither set is coplanar. Thus as a particular case we may write either

$$\phi\rho = \Sigma\alpha\, Si\rho,$$

or

$$\phi\rho = \Sigma i S\alpha_1\rho.$$

In either case we secure the nine independent scalar coefficients which are required for the expression of the most general homogeneous strain. But forms like these are relics of the early stage of quaternion development, and (as Hamilton expressly urged) they ought to be dispensed with as soon as possible.

2. A linear and vector function is completely determined if we know its effects on each of any system of three non-coplanar unit-vectors, say α, β, γ. If its cubic have three real roots, these vectors may, if we choose be taken as the directions which it

leaves unaltered; if but one, we may take a corresponding system in the form

$$\alpha,\ \beta\cos a \pm \iota\gamma\sin a,$$

where ι is $\sqrt{-1}$. But it is preferable to keep the simpler form α, β, γ, with the understanding that β and γ may be bi-vectors, of the form just written.

3. In terms of the three roots thus designed, we may form, with the help of three arbitrary scalars (two of them bi-scalars of the form $y \pm \iota z$, if necessary) three very simple but distinct varieties of linear and vector function:—viz.

(a) Strains leaving three directions, α, β, γ or $V\beta\gamma$, $V\gamma\alpha$, $V\alpha\beta$, unaltered, so that their reciprocals have the same form.

$$S\alpha\beta\gamma\ \phi\rho = x\alpha S\beta\gamma\rho + y\beta S\gamma\alpha\rho + z\gamma S\alpha\beta\rho,$$

$$\text{with } S\alpha\beta\gamma\ \phi_1\rho = xV\beta\gamma S\alpha\rho + yV\gamma\alpha S\beta\rho + zV\alpha\beta S\gamma\rho.$$

In this case, if x, y, z are the same in each, ϕ_1 is the conjugate of ϕ.

(When $x = y = z$, these strains leave the form and position of a body unaltered; but each linear dimension is increased x fold.)

(b) Pure strains:—

$$\varpi\rho = x\alpha S\alpha\rho + y\beta S\beta\rho + z\gamma S\gamma\rho,$$

$$\text{with } \varpi_1\rho = xV\beta\gamma S\beta\gamma\rho + yV\gamma\alpha S\gamma\alpha\rho + zV\alpha\beta S\alpha\beta\rho.$$

The second of these changes the system α, β, γ, into $V\beta\gamma$, $V\gamma\alpha$, $V\alpha\beta$; while the first effects the reverse operation.

(c) Combinations of two or more, from (a) or (b), or from (a) and (b):—

Either form of (a) repeated (with altered constants) simply perpetuates the form. In $\phi\phi_1$ and $\phi_1\phi$ we have new forms, which are pure when $x : y : z$ are the same in each of the factors.

The two forms (b), in succession, give one or other of the forms (a); and, conversely, either form of (a) may be regarded as the resultant of the two forms (b) taken in the proper order. This is the main result of my former paper:—for it is obvious that, having between them twelve disposable constants, ϖ and ϖ_1 may be made to represent *any* two pure strains.

But, while $\phi\varpi$ and $\varpi\phi_1$ merely repeat the type ϖ, and $\varpi_1\phi$ and $\phi_1\varpi_1$ the type ϖ_1, we have novel forms in the combinations

$$\varpi\phi,\ \phi_1\varpi,\ \phi\varpi_1, \text{ and } \varpi_1\phi_1.$$

Many of these are useful in the solution of equations among forms; such as, for instance,

$$\chi^2 = \psi,\ \psi\chi' = \chi\psi', \text{ or } \psi\chi = \chi\psi, \ \&c.$$

where χ is to be found when ψ is given. One simple result of the above discussion, which is often of great use in such matters, is the obvious condition that two such forms shall be commutative in their successive application.

4. When two roots are imaginary, all the forms above are still real; since, when β and γ take the forms $\beta \pm \iota\gamma$, y and z must be written $y \pm \iota z$. In the forms (b), the imaginary terms cancel one another; in (a) the real terms do so, and the whole is divisible by ι.

5. Of course, with α, β, γ (as in 2. above) and three scalar constants, we can produce any form of linear and vector function. And the paper concludes with forms in which these constants are merged in a new arbitrary vector.

On Electrical Properties of Fumes proceeding from Flames and Burning Charcoal. By The Right Hon. Lord Kelvin, G.C.V.O., F.R.S., and Dr Magnus Maclean.

(Read April 5, 1897.)

§ 1. Many experimenters have investigated the electrical properties of flames and incandescent solids. The methods usually employed have been (1) to examine the electric conductivity of different parts of the flame;* (2) to measure the difference of potential between platinum wires in different positions in the same flame; † (3) to find the leakage of a charged conductor when placed near, or in view of, a flame or an incandescent solid; ‡ (4) to observe the leakage of a conductor, raised to a red or white heat, by an electric current, and electrically charged; § and (5) to observe the production of electrification or diselectrification by a glowing wire, through which a current is passing, in neighbouring insulated conductors separated from it by different gases. ||

§ 2. This short communication divides itself into three separate inquiries: (1) to test by one of our electric filters ¶ the electric quality of the fumes from different flames and burnings (this method has not, we believe, been tried before); (2) to observe the difference of potential maintained between two wires of the same metal connected with a copper plate and a zinc plate when the fumes from different flames and burnings at different distances

* Account of experiments in Wiedemann's "Lehre von der Elektricität," vol. iv. B. Carl's *Rep.*, xvii. pp. 269–294, 1881. J. J. Thomson, *Phil. Mag.*, pp. 358, 441, 1890.

† Hankel, *Phil. Mag.*, p. 542, December 1851; *Phil. Mag.*, p. 9, January 1860. Elster and Geitel, *Wied. Ann.*, vol. xvi., 1882; also *Phil. Mag.*, September 1882. Maclean and Goto, *Phil. Mag.*, August 1890.

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§ Guthrie, *Phil. Mag.*, p. 237, October 1873.

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¶ Kelvin, Maclean, Galt, "Electrification and Diselectrification of Air," *Proceedings of the Royal Society, London*, vol. lvii., February and March 1895; also B. A. Report 1895.

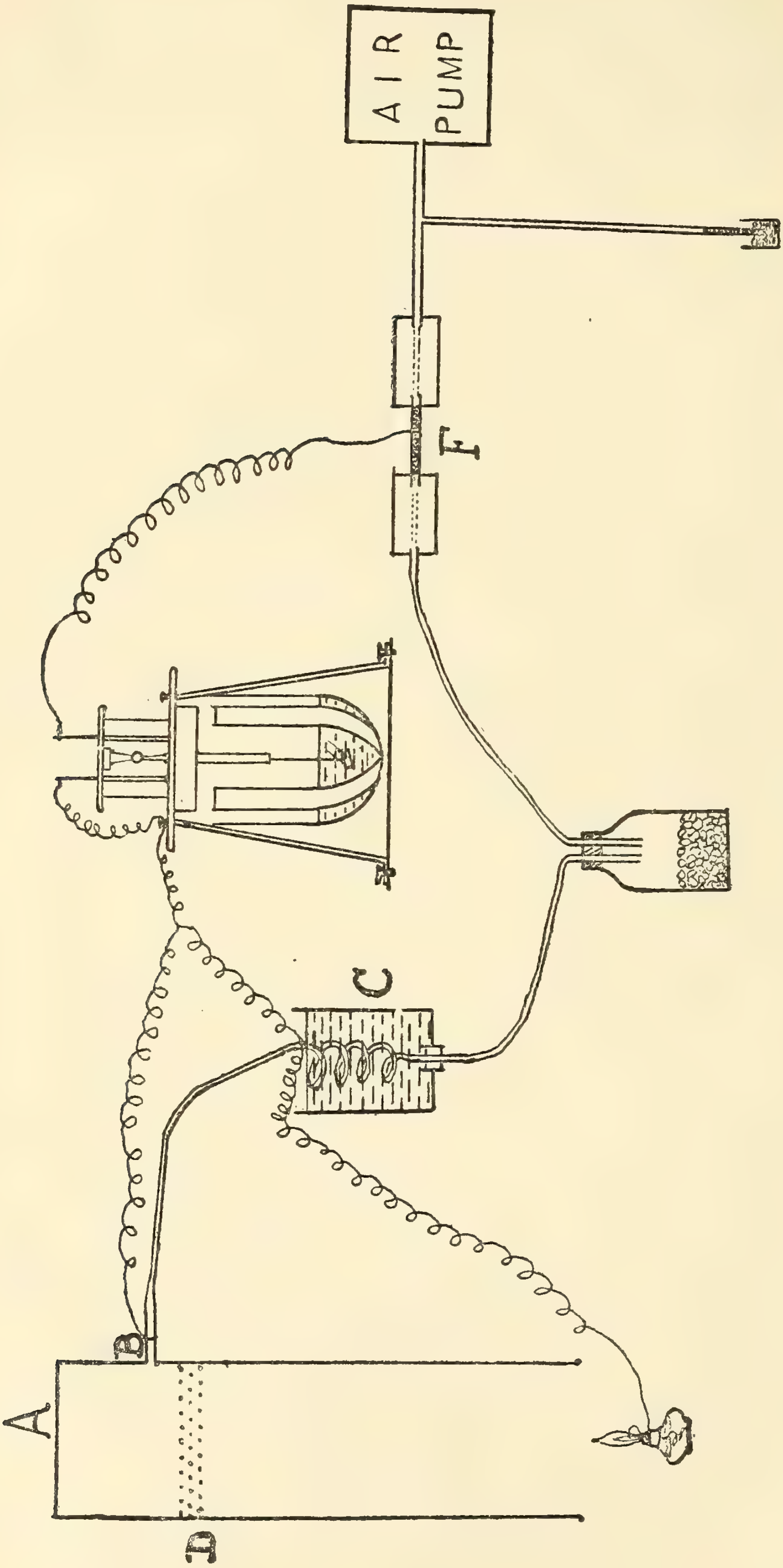


FIG. 1.

from the plates passed between them and round them; and (3) to observe the leakage between two parallel metal plates with any difference of electric potential when the fumes from flames and burnings were allowed to pass between them.

§ 3. To test the electrification of fumes from different flames and burnings, the arrangement shown diagrammatically in fig. 1 was used. The flame is kept burning at the mouth of a large vertical iron funnel A, closed at its upper end; and the heated air, along with the products of combustion, is drawn off by an air-pump through a small aperture, B, near the upper end. Before reaching the pump the air has to pass through three circular pieces of brass wire gauze, D, 1 centimetre apart, which are fixed across the funnel about 5 centimetres below the exit tube B; and through a worm of block-tin pipe, 90 centimetres long, which is kept surrounded by cold water in a vessel C. The electrification was tested by a quadrant electrometer (sensitiveness of the electrometer 111 scale divisions per volt), and an electric filter F. The filter F was of block-tin tube, 5 centimetres long and 1 centimetre bore, and full of fine brass filings kept in position by a plug of cotton wool and a piece of brass wire gauze at each end. Between the filter and the air-pump is a T-shaped piece of glass tubing with lower end of the vertical tube dipping into a basin of mercury. This served as a pressure gauge to indicate the difference of air pressures on the two sides of the filter when the air-pump was worked. The flame, the iron funnel, the worm, and the case of the electrometer are all metallically connected.

§ 4. The following flames and burnings were tried:—

(1) Candle.
(2) Paraffin lamp.
(3) Spirit flame.
(4) Portable electrometer matches.
(5) Coal-gas (Bunsen flame).
(6) Hydrogen flame.
(7) Glowing charcoal.
(8) Glowing coal.

§ 5. The method of experimenting was to place the burning substance in position at the bottom of the funnel, to insulate the

quadrant of the electrometer in connection with the electric filter, and to start working the air-pump at the rate of one stroke per three seconds. The time of each experiment was ten minutes (200 strokes of the air-pump). The results obtained are given in the following table. In testing the electrometer matches, four matches were stuck in holes in a metallic plate, and the plate connected by a wire to the case of the electrometer. These matches, according to a suggestion made more than thirty years ago by Faraday, are made of white blotting-paper soaked in a solution of nitrate of lead, and rolled up with paste into little rods of about five millimetres diameter. The hydrogen was generated in an ordinary Woulffe's bottle from zinc and hydrochloric acid. The rise of the dilute hydrochloric acid in the long vertical tube through which the acid was admitted, indicated the pressure under the nozzle, above which the hydrogen was burning.

Sensitiveness of the Electrometer 110 *scale divisions per volt.*

	Number of Experiments.	Mean Deflection in Scale Divisions of Electrometer.	Potential in Volts.
		negative.	negative.
1. One candle	2	90	0·81
2. One paraffin lamp—			
(*a*) without glass funnel . .	2	84	0·76
(*b*) with glass funnel . . .	2	30	0·27
3. One spirit lamp	4	109	0·99
4. Four portable electrometer matches	2	224	2·03
5. One Bunsen flame . . .	4	30	0·27
6. One hydrogen flame . . .	At low pressure gave small negative; at higher pressures large positive. No electrification was found from the jet at any pressure when not burning.		
7. Charcoal	Both gave negative electrification when there was a flame; and both gave positive electrification when they were glowing without flame.		
8. Coals			

§ 6. In the case of the charcoal and coal, the burning fuel was placed at the bottom of the iron funnel in a thin rectangular metallic vessel with small holes perforated in the bottom and in the sides. A wire from the case of the electrometer passed through

one of these holes, and was thrust into the burning fuel. It was noticed that when the burning charcoal was first put in position below the funnel it always produced negative electrification, which ultimately changed to positive. Thus, in four experiments, the electrification, which was at first negative, became positive after 8, 10, 14, and 18 minutes respectively. On investigation it was found that as long as any flame * was visible in the burning charcoal the electrification was negative; but as soon as all the flame disappeared, leaving only the red glow, the electrification became

* In a paper on "Electrification of Air by Combustion," by Magnus Maclean and Makita Goto, communicated to the Philosophical Society of Glasgow on November 20, 1889, is a statement of results of many observations to find the potential to which the insulated quadrant of a quadrant electrometer is raised when in metallic connection with various kinds of flames and fires. It is there said: "The effect of an ordinary lucifer match is very interesting. While the match is burning with a flame the deflection indicates positive electrification; but after the flame ceases the electrification becomes negative, the effect now being that of glowing charcoal." The following table is quoted from that paper. In some cases the burnings lasted so short a time that quantitative determinations of the potential were not obtained. It is *conceivable* that all of the complementary opposite electricity separated from that which went to the electrometer in those experiments went to uninsulated solids in the neighbourhood. The experiments described in the text demonstrate that *some of it* was lodged in the air and fumes proceeding from the fire or flame.

Substances giving Flames or Burnings.	Electrification of Insulated Fuel.	Greatest observed Potential in Volts.
Charcoal	Negative	3·0
Lucifer match, wood, and paper glowing	,,	3·0
Hydrogen	,,	0·6
Iron burning in vapour of sulphur .	,,	...
Copper ,, ,, ,, .	,,	...
Paraffin lamp	Positive	0·6
Alcohol lamp	,,	0·3
Sulphur	,,	2·0
Phosphorus exposed to air . . .	,,	1·5
Magnesium	,,	...
Iron burning in oxygen . . .	,,	...
Lucifer match, wood, and paper burning with flame	,,	...
Bisulphide of carbon	,,	0·6
Sulphuric ether	,,	0·9
Turpentine	,,	0·5
Beeswax	,,	0·7
Camphor	,,	...

positive. To test this the heated charcoal was kept away from the funnel till all flame had disappeared. Then the vessel was put in position, and the deflections obtained in two experiments were—

51 scale divisions positive in 10 minutes.
100 " " " " "

§ 7. Next an experiment was made with the burning charcoal put in position while a flame was visible. The flame remained visible for 7 minutes, and in that time a negative electrification of 34 divisions was obtained. Then the deflection came back to the metallic zero in 1 minute, and in 10 minutes more a positive electrification of 87 divisions (0·78 volts) was obtained.

§ 8. Glowing coals taken from the fire and put at once in the vessel in position, repeatedly gave negative electrification; but when they were kept away from the funnel till all flame had disappeared, the electrification obtained was slightly positive. Glowing coals remained glowing a very short time after all flame ceased, and the smallness of the observed effect is probably due to this cause.

§ 9. A few experiments have also been tried to find to what positive potential the flame must be raised so as to overcome the negative electrification it gives to the air. Hitherto the only flame tried was a spirit flame. The positive electrode of a secondary cell was put into the flame of the lamp, and the negative electrode was joined to the iron funnel and to the case of the electrometer. The results obtained are not very regular, but we found that one storage cell was not sufficient to overpower the electrifying effects of the spirit flame. With one cell we got 45 divisions negative in 10 minutes, instead of 109 divisions with metallic connection; with two cells we got 10 divisions positive in 10 minutes; and with six cells we got 83 divisions positive in 4 minutes.

§ 10. The filter, pump, and worm were now removed, and two plates—one of polished copper, and the other of polished zinc—were fixed 0·9 centimetre apart in a block of paraffin, as represented in fig. 2. The arrangement was such that either plate could be insulated, while the other was kept in metallic connection with the case of the electrometer. Observations were made to find the deflection from metallic zero with one plate insulated, and

fumes from different flames and burnings at different distances from the plates passing up between them. This may be called the fumes-zero. When the top of the flame was within 5 or 6 centimetres from the plates, the results were very irregular. The results in the following table for spirit flame are in accordance with what

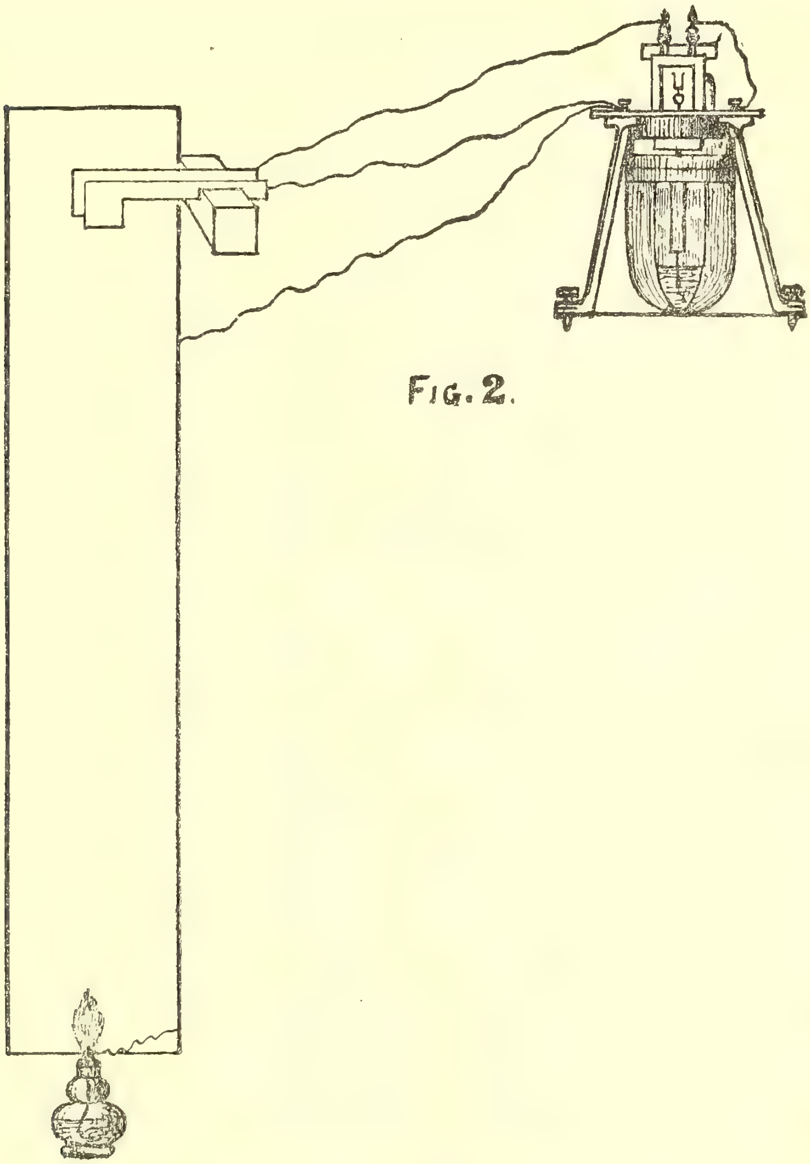


FIG. 2.

Maclean and Goto obtained from unguarded fumes from a spirit lamp 30 centimetres below the plates, as stated in their paper published in the *Philosophical Magazine* for August 1890. The effect is of the same kind as if the plates were connected by a drop of water.*

* Kelvin, *Electrostatics and Magnetism*, §§ 413, 414, pp. 332, 333.

Sensitiveness of the Electrometer 136 *scale divisions per volt.*

Flame.	Distance of top of Flame below the Plates in Centimetres.	Metal connected to Insulated Terminal of Electrometer.	Difference between Fumes Zero and Metallic Zero in Scale Divisions.	Potential in Volts.
Spirit lamp . . .	23	Copper	81 pos.	0·60
,, . . .	,,	Zinc	101 neg.	0·74
,, . . .	11	Copper	53 pos.	0·39
,, . . .	,,	Zinc	76 neg.	0·56
Paraffin lamp without glass funnel	7	Copper	141 pos.	1·04
,, ...	,,	Zinc	138 neg.	1·01
,, ...	15	Copper	90 pos.	0·66
,, ...	,,	Zinc	103 neg.	0·76
,, ...	23	Copper	108 pos.	0·79
,, ...	,,	Zinc	112 neg.	0·82
,, ...	30	Copper	83 pos.	0·61
,, ...	,,	Zinc	83 neg.	0·61

§ 11. To observe the leakage between two parallel metal plates, the zinc plate was removed, and a polished copper plate, equal and similar to the other copper plate, was substituted for it. The distance between their parallel planes was 0·9 cm. The experiments were conducted as follows: One pair of quadrants of the electrometer, with one of the copper plates in metallic connection with it, was insulated. There was now no deviation from metallic zero. A small charge, positive or negative, was given to it, producing a deflection of about 450 scale divisions. This corresponds to over 9 volts, as the sensitiveness of the electrometer now used was 48·2 scale divisions per volt. In two or three minutes the ordinary leakage of the arrangement was observed. This did not amount to more than one division, or at most two divisions, per minute. Then the flame was lit, and readings were taken every half-minute. This was done with the variations in the funnel described in the last column of the following table, and illustrated by fig. 3. For comparison, the numbers in the following table show the leakage for two minutes after the reading was 300 scale divisions (6·2 volts) from metallic zero. This gives us the leakage

at diminishing electric potentials during the time of observation. We intend to continue these experiments, and to arrange to find the leakage at different constant electric pressures.

§ 12. The marked difference in the leakage obtained when the horizontal tube was of small bore (3·8 cms.) and when it was of larger bore (15·3 cms.) may be contrasted as indicated in the last

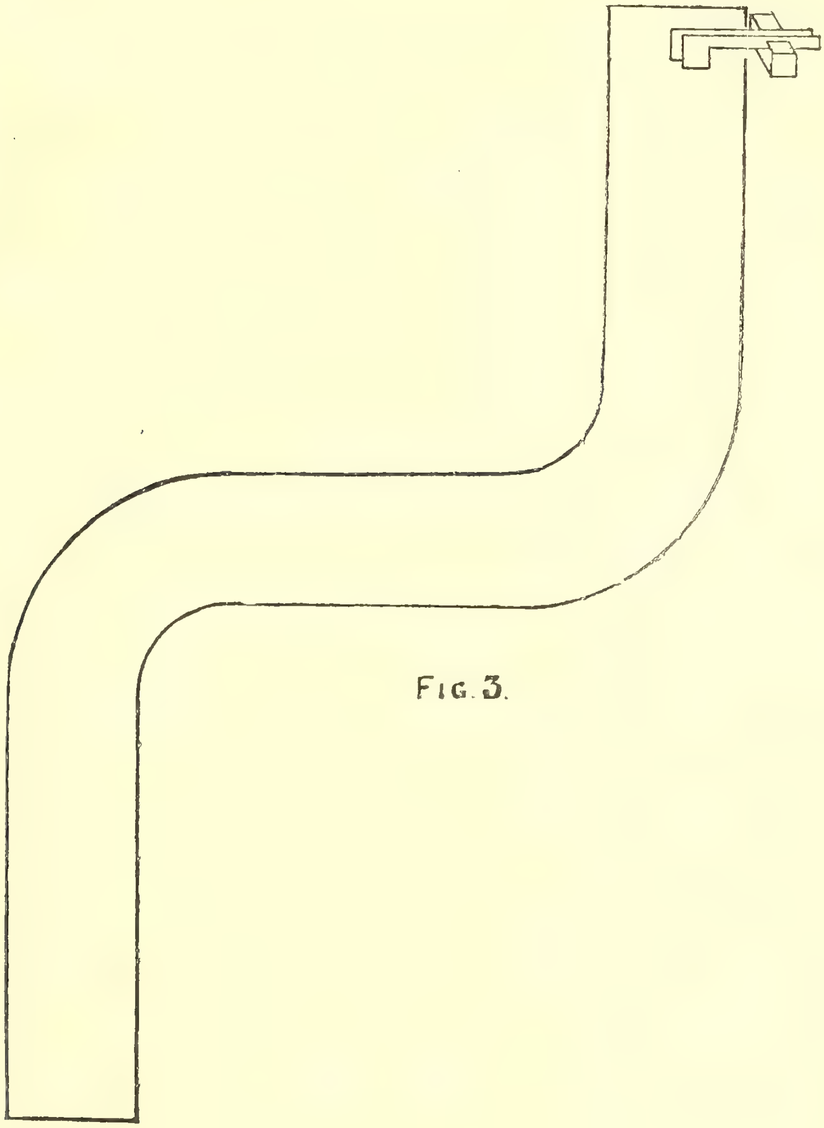


Fig. 3.

four results given for spirit flame. We also tried how long the fumes retained this conductive quality, but in every case we found that the leakage stopped in less than a quarter of a minute after the flame was extinguished, or removed from the bottom of the funnel. Closing the top and bottom of the funnel immediately after the flame was removed, we still found that the conductive quality of the air and fumes ceased within a quarter of a minute.

300 *Scale Divisions, equivalent to* 6·2 *volts, to begin with in each case.*

Flame, or Fire.	Length of Funnel between Burning Substance and Copper Plates.	Leakage in Two Minutes.	Remarks.
	Centimetres.	Scale Divisions.	
Spirit flame	66	292 pos.	Funnel of 15·3 cm. bore all vertical.
,,	,,	287 neg.	,, ,, ,,
,,	112	253 pos.	,, ,, ,,
,,	,,	254 neg.	,, ,, ,,
,,	343	22 pos.	Funnel 114 cms. vertical of 15·3 cms. bore; and 229 cms. horizontal of 3·8 cms. bore.
,,	,,	20 neg.	
,,	236	24 pos.	Same vertical, and 122 cms. horizontal of 3·8 cms. bore.
,,	,,	20 neg.	
,,	160	40 pos.	Same vertical, and 46 cms. horizontal of 3·8 cms. bore.
,,	,,	46 neg.	
,,	244	165 pos.	Same vertical, and 130 cms. horizontal of 15·3 cms. bore.
,,	,,	187 neg.	
Charcoal	,,	54 pos.	,, ,, ,,
,,	,,	57 neg.	,, ,, ,,

§ 13. In connection with these last experiments, attention may be directed to an experiment described by Prof. Schuster, in which he uses an insulated metallic tube bent round at the upper end, to prove that "it is not only the flame itself which conducts, but also the gases rising from the flame."* He discovers electric conductance in products of combustion mixed with air quite out of sight from the flame.

* Prof. Schuster, on "Atmospheric Electricity," at Royal Institution, February 22, 1895.

On Osmotic Pressure against an Ideal Semi-permeable Membrane. By The Right Hon. Lord Kelvin, LL.D., D.C.L., F.R.S.

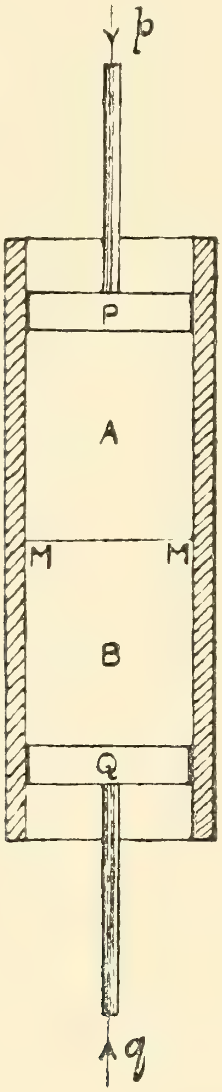
(Read January 18, 1897.)

To approach the subject of osmotic pressure against an ideal impermeable membrane, consider first a vessel filled with any particular fluid divided into two parts, A and B, by an ideal surface, MM. Let a certain number of individual molecules of the fluid in A, any one of which we shall call D (the dissolved substance), be endowed with the property that they cannot cross the surface MM (the semi-permeable membrane); but let them continue to be in other respects exactly similar to every other molecule of the fluid in A and to all the molecules of the fluid in B, any one of which we shall call S (the solvent), each of which can freely cross the membrane. Suppose now the containing vessel and the dividing membrane all perfectly rigid.* Let the apparatus be left to itself for so long time that no further change is perceptible in the progress towards final equilibrium of temperature and pressure. The pressures in A and B will be exactly the same as they would be with the same densities of the fluid if MM were perfectly impermeable, and all the molecules of the fluid were homogeneous in all qualities; and MM will be pressed on one side only, the side next A, with a force equal to the excess of the pressure in A above the pressure in B, and due solely to the impacts of D molecules striking it and rebounding from it.

If now, for a moment, we suppose the fluid to be "perfect gas," we should find the pressure on MM to be equal to that which would be produced by the D molecules if they were alone in the space A; and this is, in fact, approximately what the osmotic pressure would be with two ordinary gases at moderate pressures, one of which is confined to the space A by a membrane freely permeable by the other. On this supposition the number of the S

* In the drawing, the vessel is represented by a cylinder closed at each end by a piston to facilitate the consideration of what will happen if, instead of supposing it rigid, any arbitrary condition as to the pressures on the two sides of the membrane be imposed.

molecules per unit bulk would be the same on the two sides of the membrane. If, for example, there are 1000 S molecules to one D molecule in the space A, the pressure on the piston P would be 1001 times the osmotic pressure, and on Q 1000 times the osmotic pressure. But if the fluid be "liquid" on both sides of the membrane, we may annul the pressure on Q and reduce the pressure on P to equality with the osmotic pressure, by placing



the apparatus under the receiver of an air-pump, or by pulling Q outwards with a force equal and opposite to the atmospheric pressure on it. When we do this, the annulment of the integral pressure of the liquid on the piston Q is effected through balancing by attraction, of pressure due to impacts, between the molecules of the liquid S and the molecules of the solid piston Q. We are left absolutely without theoretical guide as to the resultant force

due to the impacts of S molecules and D molecules striking the other piston, P, and rebounding from it, and their attractions upon its molecules; and as to the numbers per unit volume of the S molecules on the two sides of MM, except that they are not generally equal. [*Addition, of date June* 30, 1897.—In an interesting article published in *Nature* of March 18, Prof. Willard Gibbs has shown that in the present ideal case the difference of pressures on the two sides of the ideal semi-permeable membrane fulfils van't Hoff's law. But this is only because of the identity of character of the S and D molecules in all qualities except in respect to action on the ideal semi-permeable membrane: and the demonstration essentially fails when the law of variation of pressure and density, according to height, differs in two vertical tubes, one of them containing S molecules alone, and the other containing a mixture of S and D molecules.]

No molecular theory can, for sugar or common salt or alcohol, dissolved in water, tell us what is the true osmotic pressure against a membrane permeable to water only, without taking into account laws quite unknown to us at present regarding the three sets of mutual attractions or repulsions: (1) between the molecules of the dissolved substance; (2) between the molecules of water; (3) between the molecules of the dissolved substance and the molecules of water. Hence van't Hoff's well-known statement, applying to solutions, Avogadro's law of gases, has manifestly no theoretical foundation at present; even though for some solutions other than mineral salts dissolved in water, it may be found somewhat approximately true; while for mineral salts dissolved in water it is wildly far from the truth. The subject is full of interest, which is increased, not diminished, by eliminating from it fallacious theoretical views. Careful consideration of how much we can really learn with certainty from theory (of which one example is the relation between osmotic pressure and vapour pressure at any one temperature) is exceedingly valuable in guiding and assisting experimental efforts for the increase of knowledge. All chemists and physicists who occupy themselves with the "theory of solutions," may well take to heart warnings, and leading views, and principles, admirably put before them by Fitzgerald in his "Helmholtz Memorial Lecture" (*Trans. Chem. Soc.*, 1896) of January 1896 (pp. 898–909).

On some Type-specimens of Lepidoptera and Coleoptera in the Edinburgh Museum of Science and Art. By Percy Hall Grimshaw, F.E.S.

(*Abstract.*)

(Read May 17, 1897.)

The paper dealt with fifty-two species of butterflies and nineteen of beetles, the type-specimens of which had been discovered by the author in a collection purchased by the University of Edinburgh from M. Dufresne of Paris in the year 1819, and afterwards transferred to the Museum of Science and Art. In the case of the butterflies, the species referred to were described by Godart in the *Encyclopédie Méthodique*, while the beetles belonged to species described by Olivier in the same work, and also in his *Histoire Naturelle des Insectes—Coléoptères*, published about the same time. By the comparison of these original specimens with others in the Natural History Collections at the British Museum the author has been enabled to clear up many points in synonymy, etc., which have for nearly eighty years remained doubtful and obscure. The most important results of the investigations may be summarised as follows :—One of the beetles has been found by Mr Gahan, of the British Museum, to be the type of a new genus, which is characterised in the present paper, while the specimen upon which it is founded is probably unique; it has been found necessary to rename one species of butterfly and one beetle; errors in synonymy have been corrected in the case of nineteen species; and eight species hitherto wrongly placed have been referred to their proper genera.

Coloured drawings have been prepared by the author of the most important of these type-specimens, and they will be reproduced in a plate accompanying the paper, which will be printed in

full in the *Transactions* of the Society. The specimens upon which the paper is founded were exhibited at the meeting.

A short paper was also read by the same author on a melanic specimen of *Hestina nama*, Dbl., very closely resembling the aberration named by Oberthür *melanina* (*Études d'Entomologie*, xx., 1896, p. 30, pl. x., No. 177). This communication, with a coloured drawing of the insect, will also be issued in the *Transactions.*

The Eliminant of a Set of Quaternary Quadrics. By Thomas Muir, LL.D.

(Read December 7, 1896.)

1. The "dialytic" method of elimination, in the case of more than one variable, is not without its drawbacks, as most mathematicians know. The requisite derived equations are not always easily obtained, the difficulty being due as often to the existence of too many as of too few; and, when this has been got over, it not unfrequently happens that the order of the resulting determinant is alarmingly high, and unaccompanied by any hope of a successful guess as to the character of the extraneous factor. The discoverer's original paper * affords sufficient testimony of this, and very little has been done since its appearance to put matters on a sounder footing. The most noteworthy improvement, due to Cayley,† is more interesting theoretically than practically, his main object being the detection of the extraneous factor when there is an overplus of equations. Nothing, indeed, will be found more conducive to an understanding of the limitations of the method than a careful comparison of the application of this process of Cayley's to the problem of eliminating x, y, z from the three ternary quadrics

$$\left.\begin{array}{l} a_1x^2 + b_1y^2 + c_1z^2 + l_1yz + m_1zx + n_1xy = 0 \\ a_2x^2 + b_2y^2 + c_2z^2 + l_2yz + m_2zx + n_2xy = 0 \\ a_3x^2 + b_3y^2 + c_3z^2 + l_3yz + m_3zx + n_3xy = 0 \end{array}\right\}$$

with Sylvester's original treatment of the same problem, and the latter especially as commented on in footnotes by the author himself.

2. The want of definiteness in the mode of arriving at the exact

* Sylvester, "Examples of the Dialytic Method of Elimination as applied to Ternary Systems of Equations," *Camb. Math. Journ.*, ii. (1841), pp. 232–236.

† Cayley, "On the Theory of Elimination," *Camb. and Dub. Math. Journ.*, iii. (1848), pp. 116–120; or *Collected Math. Papers*, i. pp. 370–374.

number of derived equations is not without its compensating advantage, as it leaves that scope for the exercise of ingenuity which is half the charm of mathematical work. The variety of forms, too, which it is often possible to obtain for the same eliminant is a matter of great interest, and the study of them is almost always certain to have an instructive result. This appears very clearly from the work recently devoted to the elucidation of one of Sylvester's cases,—the first case of all, in fact, and therefore classical. It consists in the elimination of x, y, z from the equations

$$\left.\begin{array}{l} Ay^2 - 2C'xy + Bx^2 = 0 \\ Bz^2 - 2A'yz + Cy^2 = 0 \\ Cx^2 - 2B'zx + Az^2 = 0 \end{array}\right\} ,$$

where there being six secondary variables, x^2, y^2, z^2, yz, zx, xy, it was necessary to seek for three other equations containing the same variables and not derivable from the original three by mere addition or subtraction of multiples. The three ingeniously arrived at were

$$\left.\begin{array}{l} C'z^2 + Cxy - A'zx - B'yz = 0 \\ A'x^2 + Ayz - B'xy - C'zx = 0 \\ B'y^2 + Bzx - C'yz - A'xy = 0 \end{array}\right\} ;$$

but, when they had been got, the resulting determinant was, of course, of the 6th order, and, on investigation, it proved to be twice the square of the real eliminant.

3. This same case of Sylvester's turns out now to be most instructive from a totally different point of view. On first thoughts, it would seem as if nothing could be easier than the generalisation of the problem from the case of three variables to four. Yet such is very far from being the truth, and an excellent illustration is thus afforded of another peculiarity of the method, viz., its apparent capriciousness.

Consider the four equations

$$\left.\begin{array}{ll} Bx^2 - Dxy + Ay^2 = 0 & (1) \\ Cy^2 - Eyz + Bz^2 = 0 & (2) \\ Lz^2 - Kzw + Cw^2 = 0 & (3) \\ Aw^2 - Gwx + Lx^2 = 0 & (4) \end{array}\right\}$$

and follow Sylvester closely. There being now *eight* "secondary" variables, as we may call them, viz., x^2, y^2, z^2, w^2, xy, yz, zw, wx, we have to seek for four additional equations containing them. Employing Sylvester's process, we first eliminate B from equations (1) and (2), with the result

$$-\mathrm{D}xyz^2 + \mathrm{A}y^2z^2 - \mathrm{C}y^2x^2 + \mathrm{E}yzx^2 = 0\,;$$

and then L from equations (3) and (4), with the result

$$-\mathrm{K}zwx^2 + \mathrm{C}w^2x^2 - \mathrm{A}w^2z^2 + \mathrm{G}wxz^2 = 0\,.$$

On proceeding, however, to eliminate C from these two equations, we find that this will cause the elimination of A also, the result in fact being

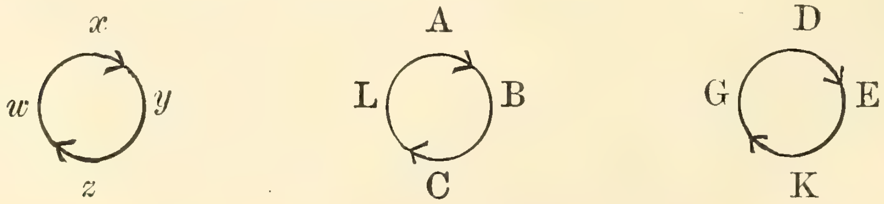
$$-\mathrm{D}xyz^2w^2 + \mathrm{E}yzx^2w^2 - \mathrm{K}zwx^2y^2 - \mathrm{G}wxz^2y^2 = 0\,,$$

or

$$\mathrm{D}zw - \mathrm{E}wx + \mathrm{K}xy - \mathrm{G}yz = 0\,. \qquad (\alpha).$$

Here the trouble begins. In Sylvester's case the first derived equation contained not only all the secondary variables of the type xy, but also one of the type x^2, the consequence being that, by repeating the process on a different arrangement of the original equations, he obtained a new derived equation, and, of course, similarly a third. In the case we are now dealing with this is impossible of attainment, the manipulation of the original equations in a different order leading invariably to the same result. Instead of being, as in Sylvester's case, one of a set, the equation (α) is here unique.

This fact comes out very clearly and conclusively in another way. It will be observed that any one of the four original equations is derivable from another of them by changing the letters in accordance with the indications of the cycles



Consequently, if by any legitimate process an equation is deduced from two or more of the original equations, the process need not be repeated in order to obtain other like deduced equations; all that

is necessary is a mere change of lettering. Now, whereas in Sylvester's case this letter-changing was fruitful, in the new case it is not. The equation (α) here simply reproduces itself; in other words, it is invariant to the cyclical substitution.

4. Instead of eight equations we have thus only five, and cannot therefore proceed. Of course some process of derivation different from Sylvester's may lead to four new equations instead of one, but I know as yet of no such process. True, there is one other equation outwardly resembling (α), viz.,

$$\mathrm{CDL}xy - \mathrm{LEA}yz + \mathrm{AKB}zw - \mathrm{BGC}wx = 0 \qquad (\beta);$$

but this, unfortunately, is useless for dialytic purposes when taken along with the given set, being obtainable from them by mere addition and subtraction of multiples, the operation which gives it being

$$-\mathrm{CL}(1) + \mathrm{LA}(2) - \mathrm{AB}(3) + \mathrm{BC}(4).$$

It corresponds, in fact, to an equation first derived from Sylvester's set in my paper of 1892 (*Proc. Roy. Soc. Edin.*, xx. pp. 300–305). It is not unworthy of note in passing, however, that here again there is a difference between the analogues. The corresponding equation in Sylvester's case is accompanied by two others. Here (β) is unique, being invariant to the cyclical substitution.

5. Such being the state of matters, we are apparently forced to go afield for a fresh mode of dialytic elimination. One such need only be mentioned to be dismissed.

From each of the five equations, (1), (2), (3), (4), (α), by multiplying in succession by w, x, y, z, we can derive four equations, and, therefore, can have in all twenty. But the secondary variables in these would be

$$\begin{array}{cccc} x^3, & y^3, & z^3, & w^3 \\ x^2y, & y^2z, & z^2w, & w^2x \\ x^2z, & y^2w, & z^2x, & w^2y \\ x^2w, & y^2x, & z^2y, & w^2z \\ yzw, & xzw, & xyw, & xyz; \end{array}$$

and these being also twenty in number, dialytic elimination is possible.

A less forbidding result is obtained by confining oneself to the secondary variables

$$\begin{array}{cccc} x^2y, & y^2z, & z^2w, & w^2x \\ x^2w, & y^2x, & z^2y, & w^2z \\ yzw, & xzw, & xyw, & xyz. \end{array}$$

To do so, we perform the operation

$$\mathrm{L}x(1) - \mathrm{B}x(4),$$

the result being

$$\mathrm{LA}xy^2 - \mathrm{LD}x^2y - \mathrm{BA}xw^2 + \mathrm{BG}x^2w = 0,$$

and three others from it. Next, we perform the operation

$$\mathrm{L}z(1) - \mathrm{B}z(4),$$

the result being

$$\mathrm{LA}y^2z - \mathrm{LD}xyz - \mathrm{BA}zw^2 + \mathrm{BG}xzw = 0,$$

and three others from it. Lastly, we perform the operations

$$x(a), \quad y(a), \quad z(a), \quad w(a).$$

We thus obtain in all twelve equations; but the resulting determinant, although now of only the 12th order, is still of the 20th degree in the coefficients A, B, C, L, D, etc.

6. These attempts are neither new as to mode nor satisfactory in the result. The same, however, can scarcely be said of the process now to be given, where the secondary variables are much more complicated in appearance, viz.,

$$\frac{\mathrm{C}x^2 + \mathrm{A}z^2}{xz} \cdot \frac{\mathrm{L}y^2 + \mathrm{B}w^2}{yw}, \qquad \frac{\mathrm{C}x^2 + \mathrm{A}z^2}{xz}, \qquad \frac{\mathrm{L}y^2 + \mathrm{B}w^2}{yw},$$

or, for shortness' sake, say

$$\phi\theta, \quad \phi, \quad \theta,$$

in connection with which it must be carefully observed that the result of the cyclical substitution is to change

$$\phi \text{ into } \theta,$$
$$\text{and } \theta \text{ into } \phi.$$

The equation which has been obtained connecting these variables is

$$\mathrm{D}\cdot\phi\theta - 2\mathrm{BG}\cdot\phi - 2\mathrm{AE}\cdot\theta + (4\mathrm{ABK} + \mathrm{DEG} - \mathrm{D}^2\mathrm{K}) = 0,$$

the verification of it being readily effected by substituting for

D, E, K, G their values as found from the original equations. Or, we may proceed synthetically, thus:—

$$\text{From } (\beta)\ \mathrm{AKB}xyzw = -\mathrm{CDL}x^2y^2 + \mathrm{LEA}xy^2z + \mathrm{BGC}x^2yw,$$
$$\text{and from } (\alpha)\ \mathrm{AKB}xyzw = \mathrm{ABG}yz^2w - \mathrm{ABD}z^2w^2 + \mathrm{ABE}xzw^2\,;$$

∴ by addition

$$\begin{aligned}4\mathrm{AKB}xyzw = &-2\mathrm{CDL}x^2y^2 - 2\mathrm{ABD}z^2w^2 \\ &+ 2\mathrm{ABG}yz^2w + 2\mathrm{LEA}xy^2z + 2\mathrm{BGC}x^2yw + 2\mathrm{ABE}xzw^2.\end{aligned}$$

$$\text{Again, from } (1),(3)\ \ \mathrm{DK}xyzw = \mathrm{BL}x^2z^2 + \mathrm{AL}y^2z^2 + \mathrm{BC}x^2w^2 + \mathrm{AC}y^2w^2,$$
$$\text{and from } (2),(4)\ \ \mathrm{EG}xyzw = \mathrm{BL}x^2z^2 + \mathrm{CL}x^2y^2 + \mathrm{AB}z^2w^2 + \mathrm{AC}y^2w^2\,;$$

∴ by addition

$$\mathrm{D}(\mathrm{EG} - \mathrm{DK})xyzw = \mathrm{D}(\mathrm{CL}x^2y^2 + \mathrm{AB}z^2w^2 - \mathrm{AL}y^2z^2 - \mathrm{BC}x^2w^2)\,.$$

From this and the previous result it follows that

$$\begin{aligned}(4\mathrm{AKB} + \mathrm{DEG} - \mathrm{D}^2\mathrm{K})\cdot xyzw &= -\mathrm{D}(\mathrm{CL}x^2y^2 + \mathrm{AB}z^2w^2 + \mathrm{AL}y^2z^2 + \mathrm{BC}x^2w^2) \\ &\quad + 2\mathrm{ABG}yz^2w + \ \ldots\ldots \\ &= -\mathrm{D}(\mathrm{C}x^2 + \mathrm{A}z^2)\,(\mathrm{L}y^2 + \mathrm{B}w^2) \\ &\quad + 2\mathrm{BG}(\mathrm{C}x^2 + \mathrm{A}z^2)yw + 2\mathrm{AE}(\mathrm{L}y^2 + \mathrm{B}w^2)xz\,,\end{aligned}$$

which, on division by $xyzw$, gives as desired

$$\mathrm{D}\cdot\phi\theta - 2\mathrm{BG}\cdot\phi - 2\mathrm{AE}\cdot\theta + (4\mathrm{AKB} + \mathrm{DEG} - \mathrm{D}^2\mathrm{K}) = 0\,.$$

This is not invariable to the cyclical substitution, but leads at once to the three others

$$\begin{aligned}&\mathrm{E}\cdot\theta\phi - 2\mathrm{CD}\cdot\theta - 2\mathrm{BK}\cdot\phi + (4\mathrm{BGC} + \mathrm{EKD} - \mathrm{E}^2\mathrm{G}) = 0 \\ &\mathrm{K}\cdot\phi\theta - 2\mathrm{LE}\cdot\phi - 2\mathrm{CG}\cdot\theta + (4\mathrm{CDL} + \mathrm{KGE} - \mathrm{K}^2\mathrm{D}) = 0 \\ &\mathrm{G}\cdot\theta\phi - 2\mathrm{AK}\cdot\theta - 2\mathrm{LD}\cdot\phi + (4\mathrm{LEA} + \mathrm{GDK} - \mathrm{G}^2\mathrm{E}) = 0\,,\end{aligned}$$

whence, on the elimination of $\phi\theta$, ϕ, θ, we have

$$\begin{vmatrix} \mathrm{D} & \mathrm{BG} & \mathrm{AE} & 4\mathrm{AKB} + \mathrm{D}(\mathrm{EG} - \mathrm{DK}) \\ \mathrm{E} & \mathrm{BK} & \mathrm{CD} & 4\mathrm{BGC} + \mathrm{E}(\mathrm{DK} - \mathrm{EG}) \\ \mathrm{K} & \mathrm{LE} & \mathrm{CG} & 4\mathrm{CDL} + \mathrm{K}(\mathrm{EG} - \mathrm{DK}) \\ \mathrm{G} & \mathrm{LD} & \mathrm{AK} & 4\mathrm{LEA} + \mathrm{G}(\mathrm{DK} - \mathrm{EG}) \end{vmatrix} = 0\,,$$

or

$$\begin{vmatrix} \mathrm{D} & \mathrm{BG} & \mathrm{AE} & \mathrm{K}(2\mathrm{AB} - \mathrm{D}^2) \\ \mathrm{E} & \mathrm{BK} & \mathrm{CD} & \mathrm{G}(2\mathrm{BC} - \mathrm{E}^2) \\ \mathrm{K} & \mathrm{LE} & \mathrm{CG} & \mathrm{D}(2\mathrm{CL} - \mathrm{K}^2) \\ \mathrm{G} & \mathrm{LD} & \mathrm{AK} & \mathrm{E}(2\mathrm{LA} - \mathrm{G}^2) \end{vmatrix} = 0\,,$$

or

$$\overset{\circ}{\Sigma}A^2B^2K^4 - DEKG\cdot\overset{\circ}{\Sigma}ABK^2 - 2ABCL\overset{\circ}{\Sigma}G^2E^2$$
$$- 2\overset{\circ}{\Sigma}ACB^2K^2G^2 + \overset{\circ}{\Sigma}ABE^2K^2G^2 + 8ABCL\cdot DEKG = 0\,;$$

where $\overset{\circ}{\Sigma}$ indicates cyclical summation, four terms being always included in the sum, except in the case of $\overset{\circ}{\Sigma}G^2E^2$, which is used to stand for $G^2E^2 + D^2K^2$, and not for $G^2E^2 + D^2K^2 + E^2G^2 + K^2D^2$, as it might well do.

7. In Sylvester's original paper he explained, in passing, that his process of solution had been suggested to him from a consideration of the problem of finding the discriminant of the ternary quadric

$$Ax^2 + By^2 + Cz^2 + 2A'yz + 2B'zx + 2C'xy\,,$$

and in my paper above referred to the nature of the relation between the two problems is attempted to be made clear. It is, therefore, not strange that the solution of the preceding paragraph should be due to a similar suggestion.

If the quaternary quadric

$$A\xi_1{}^2 + B\xi_2{}^2 + C\xi_3{}^2 + L\xi_4{}^2 + D\xi_1\xi_2 + E\xi_2\xi_3 + F\xi_3\xi_1 + G\xi_1\xi_4 + H\xi_2\xi_4 + K\xi_3\xi_4$$

be the product of two linear factors,

$$(\alpha_1\xi_1 + \beta_1\xi_2 + \gamma_1\xi_3 + \delta_1\xi_4)\,,\ \ (\alpha_2\xi_1 + \beta_2\xi_2 + \gamma_2\xi_3 + \delta_2\xi_4)\,,$$

the ten equations

$$\begin{array}{lll} \alpha_1\alpha_2 = A\,, & \alpha_1\beta_2 + \alpha_2\beta_1 = D\,, & \beta_1\gamma_2 + \beta_2\gamma_1 = E\,, \\ \beta_1\beta_2 = B\,, & \alpha_1\gamma_2 + \alpha_2\gamma_1 = F\,, & \beta_1\delta_2 + \beta_2\delta_1 = H\,, \\ \gamma_1\gamma_2 = C\,, & \alpha_1\delta_2 + \alpha_2\delta_1 = G\,, & \gamma_1\delta_2 + \gamma_2\delta_1 = K\,, \\ \delta_1\delta_2 = L\,, & & \end{array}$$

will hold, and as consequences of these, the 10 principal minors of the determinant

$$\begin{vmatrix} 2A & D & F & G \\ D & 2B & E & H \\ F & E & 2C & K \\ G & H & K & 2L \end{vmatrix}$$

will vanish. That is to say, we shall have

(1) an equation connecting A, B, C, , D, E, F, , , ;
(2) " " A, B, , L, D, , , G, H, ;
(3) " " A, , C, L, , , F, G, , K;

(4) an equation connecting , B, C, L, , E, , , H, K;
(5) ,, ,, A, B, , , D, E, F, G, H, K;
(6) ,, ,, , B, C, , D, E, F, G, H, K;
(7) ,, ,, , , C, L, D, E, F, G, H, K;
(8) ,, ,, A, , , L, D, E, F, G, H, K;
(9) ,, ,, A, , C, , D, E, F, G, H, K;
(10) ,, ,, , B, , L, D, E, F, G, H, K.

Now these must be derivable either from a set of six or eight of the ten equations $\alpha_1\alpha_2 = A$, etc. For example, the first,—connecting A, B, C, D, E, F,—must be derivable from the six in which these letters occur, viz.,

$$\begin{aligned} \alpha_1\alpha_2 &= A\,, & \alpha_1\beta_2 + \alpha_2\beta_1 &= D\,, \\ \beta_1\beta_2 &= B\,, & \beta_1\gamma_2 + \beta_2\gamma_1 &= E\,, \\ \gamma_1\gamma_2 &= C\,, & \gamma_1\alpha_2 + \gamma_2\alpha_1 &= F\,. \end{aligned}$$

That such is the case is readily seen by primarily eliminating only α_2, β_2, γ_2, when there remain the three equations

$$\begin{aligned} B\alpha_1^2 + A\beta_1^2 &= D\alpha_1\beta_1 \\ C\beta_1^2 + B\gamma_1^2 &= E\beta_1\gamma_1 \\ A\gamma_1^2 + C\alpha_1^2 &= F\gamma_1\alpha_1\,, \end{aligned}$$

from which, on using Sylvester's first case,—as must be carefully remarked,—we have

$$\begin{vmatrix} 2A & D & F \\ D & 2B & E \\ F & E & 2C \end{vmatrix} = 0\,,$$

as was expected. Similarly, the relation connecting the eight coefficients A, B, , , D, E, F, G, H, K must be derivable from the eight equations

$$\begin{aligned} \alpha_1\alpha_2 &= A\,, & \alpha_1\gamma_2 + \alpha_2\gamma_1 &= F\,, \\ \beta_1\beta_2 &= B\,, & \alpha_1\delta_2 + \alpha_2\delta_1 &= G\,, \\ \alpha_1\beta_2 + \alpha_2\beta_1 &= D\,, & \beta_1\delta_2 + \beta_2\delta_1 &= H\,, \\ \beta_1\gamma_2 + \beta_2\gamma_1 &= E\,, & \gamma_1\delta_2 + \gamma_2\delta_1 &= K\,. \end{aligned}$$

To make the necessary test we may first eliminate α_2, β_2, the result being the six equations

$$\begin{aligned} B\alpha_1^2 + A\beta_1^2 &= D\alpha_1\beta_1\,, & \alpha_1^2\delta_2 + A\delta_1 &= G\alpha_1\,, \\ \beta_1^2\gamma_2 + B\gamma_1 &= E\beta_1\,, & \beta_1^2\delta_2 + B\delta_1 &= H\beta_1\,, \\ \alpha_1^2\gamma_2 + A\gamma_1 &= F\alpha_1\,, & \gamma_1\delta_2 + \gamma_1\delta_2 &= K\,. \end{aligned}$$

Solving the 2nd and 3rd for γ_1, γ_2, and the 4th and 5th for δ_1, δ_2, we find on substitution in the 6th

$$\mathrm{KB}^2\alpha_1{}^4 - (\mathrm{BEG}+\mathrm{BFH})\alpha_1{}^3\beta_1 + 2(\mathrm{AEH}-\mathrm{ABK}+\mathrm{BFG})\alpha_1{}^2\beta_1{}^2 - (\mathrm{AHF}+\mathrm{AEG})\alpha_1\beta_1{}^3 + \mathrm{KA}^2\beta_1{}^4 = 0\,,$$

which, when taken along with the remaining equation, gives

$$\mathrm{KD}^2 - 4\mathrm{ABK} - \mathrm{DEG} - \mathrm{DFH} + 2\mathrm{AEH} + 2\mathrm{BFG} = 0\,,$$

or

$$\begin{vmatrix} 2\mathrm{A} & \mathrm{D} & \mathrm{G} \\ \mathrm{D} & 2\mathrm{B} & \mathrm{H} \\ \mathrm{F} & \mathrm{E} & \mathrm{K} \end{vmatrix} = 0\,,$$

as it should be.

In the next place, however, it is important to notice that there are more than 10 such relations connecting the coefficients, A, B, C, L, D, etc. For, as the preceding mode of obtaining the relations shows, we have only got to select a certain number of the 10 equations $\alpha_1\alpha_2 = \mathrm{A}$, etc., from which it may be possible to eliminate the Greek letters, and one such relation must result. To take at once the most pertinent example, let us choose the eight equations

$$\begin{array}{rr} \alpha_1\alpha_2 = \mathrm{A}\,, & \alpha_1\beta_2 + \beta_1\alpha_2 = \mathrm{D}\,, \\ \beta_1\beta_2 = \mathrm{B}\,, & \beta_1\gamma_2 + \beta_2\gamma_1 = \mathrm{E}\,, \\ \gamma_1\gamma_2 = \mathrm{C}\,, & \gamma_1\delta_2 + \gamma_2\delta_1 = \mathrm{K}\,, \\ \delta_1\delta_2 = \mathrm{L}\,, & \delta_1\alpha_2 + \delta_2\alpha_1 = \mathrm{G}\,. \end{array}$$

From these, on eliminating α_2, β_2, γ_2, δ_2, we have the four equations

$$\begin{aligned} \mathrm{B}\alpha_1{}^2 + \mathrm{A}\beta_1{}^2 &= \mathrm{D}\alpha_1\beta_1\,, \\ \mathrm{C}\beta_1{}^2 + \mathrm{B}\gamma_1{}^2 &= \mathrm{E}\beta_1\gamma_1\,, \\ \mathrm{L}\gamma_1{}^2 + \mathrm{C}\delta_1{}^2 &= \mathrm{K}\gamma_1\delta_1\,, \\ \mathrm{A}\delta_1{}^2 + \mathrm{L}\alpha_1{}^2 &= \mathrm{G}\delta_1\alpha_1\,, \end{aligned}$$

and there thus remains the problem of eliminating α_1, β_1, γ_1, δ_1, from these four. But this is the very problem we have been considering as the extension of Sylvester's, and it is its appearance in this connection which at once suggests the possibility of attacking it in the rear. To begin with, we infer that the new relation sought cannot be an independent relation, but must be derivable from two or more of the initial ten, only three of which, by the way, are themselves independent. Now, when we examine the ten carefully, we find that there is a set of four which involve all the co-

efficients, but from which the two not desired may be eliminated. These are

$$\begin{vmatrix} 2A & D & F \\ D & 2B & E \\ G & H & K \end{vmatrix} \quad \text{or} \quad DFH - 2BGF - 2AEH + 4ABK + DEG - D^2K = 0\,,$$

$$\begin{vmatrix} D & 2B & E \\ F & E & 2C \\ G & H & K \end{vmatrix} \quad \text{or} \quad EFH - 2BKF - 2CDH + 4BCG + DEK - GE^2 = 0\,,$$

$$\begin{vmatrix} D & E & H \\ F & 2C & K \\ G & K & 2L \end{vmatrix} \quad \text{or} \quad KFH - 2ELF - 2CGH + 4CDL + GEK - DK^2 = 0\,,$$

$$\begin{vmatrix} 2A & D & G \\ F & E & K \\ G & H & 2L \end{vmatrix} \quad \text{or} \quad GFH - 2DLF - 2AKH + 4AEL + DGK - EG^2 = 0\,,$$

where it is clear that we have the means of eliminating F and H and obtaining a relation connecting A, B, C, L, D, E, K, G. The thought therefore occurs to one, that if it were possible to deduce four equations like these from the four given equations, our difficulty would be overcome. Of course, the deduced equations could not be exactly like these, for they could not possibly contain the letters F and H. But any quantities whatever that might occur as F and H do occur would suit our purpose equally well, because we only want them in order to eliminate them. And here another suggestion comes in, viz., that as the equivalents of F and H in the set of ten equations, $\alpha_1\alpha_2 = A$, etc., are

$$\begin{array}{llcl} & \alpha_1\gamma_2 + \alpha_2\gamma_1 & \text{and} & \beta_1\delta_2 + \beta_2\delta_1\,, \\ \text{or} & \alpha_1 . \dfrac{C}{\gamma_1} + \dfrac{A\gamma_1}{\alpha_1} & \text{and} & \beta_1 . \dfrac{L}{\delta_1} + \dfrac{B\delta_1}{\beta_1}\,, \\ \text{or} & \dfrac{C\alpha_1^2 + A\gamma_1^2}{\alpha_1\gamma_1} & \text{and} & \dfrac{L\beta_1^2 + B\delta_1^2}{\beta_1\delta_1}\,, \end{array}$$

it is almost certain that these will suit. As a matter of fact, on turning to the solution which was obtained (§ 6), it will be found that they are exactly those which were taken and found to suit.

8. These suggestions, however, are not all that are obtainable from the vanishing of the ten primary minors of the discriminant.

Among the remaining six relations we find two that are independent of H but are quadratics in F, and two that are independent of F but are quadratics in H. The elimination of F from the first pair and the elimination of H from the second pair ought, one would think, to lead us to one and the same resultant, and ought, indeed, to give us the very resultant obtained previously from the set of four relations. On trial this is found to be the case; consequently, we shall obtain a new mode of arriving at the eliminant of our set of equations

$$\left.\begin{aligned} Bx^2 - Dxy + Ay^2 = 0 \\ Cy^2 - Eyz + Bz^2 = 0 \\ Lz^2 - Kzw + Cw^2 = 0 \\ Aw^2 - Gwx + Lx^2 = 0 \end{aligned}\right\}$$

if from them we can deduce the equation

$$A\left(\frac{Ly^2 + Bw^2}{yw}\right)^2 - DG\left(\frac{Ly^2 + Bw^2}{yw}\right) + (BG^2 + D^2L - 4ABL) = 0$$

the latter being suggested by one of the above-mentioned quadratics in H, viz.,

$$-\tfrac{1}{2}\begin{vmatrix} 2A & D & G \\ D & 2B & H \\ G & H & 2L \end{vmatrix} \quad \text{or} \quad AH^2 - DGH + BG^2 + D^2L - 4ABL = 0\,.$$

Fortunately, the required deduction is perfectly easily made. We may either, as before, simply substitute for D, E, G their values derived from the given equations, or we may improve upon this, as follows:—

Starting with the second term we see that

$$\begin{aligned} &DG(Ly^2 + Bw^2)yw \\ &= Dy \cdot Gw(Ly^2 + Bw^2)\,, \\ &= \frac{Bx^2 + Ay^2}{x} \cdot \frac{Aw^2 + Lx^2}{x} \cdot (Ly^2 + Bw^2)\,, \\ &= (ABw^2 + A^2\frac{y^2w^2}{x^2} + BLx^2 + ALy^2)(Ly^2 + Bw^2)\,, \\ &= A(Ly^2 + Bw^2)^2 + (A^2\frac{y^2w^2}{x^2} + BLx^2)(Ly^2 + Bw^2)\,; \end{aligned}$$

treating similarly the 3rd term we have

$$
\begin{aligned}
&(BG^2 + D^2L - 4ABL)y^2w^2 \\
&= By^2(Gw)^2 + Lw^2(Dy)^2 - 4ABLy^2w^2, \\
&= By^2\left(\frac{Aw^2 + Lx^2}{x}\right)^2 + Lw^2\left(\frac{Bx^2 + Ay^2}{x}\right)^2 - 4ABLy^2w^2, \\
&= BL^2x^2y^2 + BA^2\frac{y^2w^4}{x^2} + LB^2w^2x^2 + LA^2\frac{w^2y^4}{x^2}, \\
&= (Ly^2 + Bw^2)\left(BLx^2 + A^2\frac{y^2w^2}{x^2}\right);
\end{aligned}
$$

and, finally, from the two results by subtraction there emerges the desired equation.

Using θ and ϕ as before, and taking advantage of the cyclical substitution, we thus have

$$
\begin{aligned}
&A\theta^2 - DG\theta + (BG^2 + D^2L - 4ABL) = 0, \\
&B\phi^2 - ED\phi + (CD^2 + E^2A - 4BCA) = 0, \\
&C\theta^2 - KE\theta + (LE^2 + K^2B - 4CLB) = 0, \\
&L\phi^2 - GK\phi + (AK^2 + G^2C - 4LAC) = 0.
\end{aligned}
$$

From the 1st and 3rd of these there results the eliminant

$$
\begin{vmatrix} A & BG^2 + D^2L - 4ABL \\ C & LE^2 + K^2B - 4BCL \end{vmatrix}^2 - \begin{vmatrix} A & DG \\ C & KE \end{vmatrix} \cdot \begin{vmatrix} DG & BG^2 + D^2L - 4ABL \\ KE & LE^2 + K^2B - 4CLB \end{vmatrix},
$$

or

$$
\begin{aligned}
(ALE^2 + ABK^2 - BCG^2 - CLD^2)^2 - 4BL(AEK - CDG)^2 \\
- (AEK - CDG)(EG - DK)(DEL - BGK),
\end{aligned}
$$

or

$$
\begin{aligned}
\mathring{\Sigma} A^2B^2K^4 - 2\mathring{\Sigma} ACB^2K^2G^2 - 2ABCL\mathring{\Sigma} G^2E^2 + 8ABCL \cdot DEKG \\
- DEKG\mathring{\Sigma} ABK^2 + \mathring{\Sigma} ABE^2K^2G^2,
\end{aligned}
$$

which agrees completely with the result of § 6.

From the 2nd and 4th, or simply by cyclical substitution, we obtain the eliminant also in the form

$$
\begin{aligned}
(-ALE^2 + ABK^2 + BCG^2 - CLD^2)^2 - 4CA(BKG - DEL)^2 \\
- (AEK - CDG)(EG - DK)(DEL - BGK),
\end{aligned}
$$

where the last term, being invariant to the cyclical substitution, is the same as before, and where therefore the two other terms, not being individually invariant, must be so when taken together.

9. These modes of solving our problem suggest further to us that, by ringing the changes in selecting eight of the ten equations $\alpha_1\alpha_2 = A$, etc., we may obtain a variety of new problems in elimination, the eliminants of which may be foretold, so to speak, by keeping an eye on the ten vanishing minors of the related discriminant. Thus we may choose the eight equations

$$\begin{array}{lll} \beta_1\beta_2 = B, & \alpha_1\beta_2 + \alpha_2\beta_1 = D, & \beta_1\gamma_2 + \beta_2\gamma_1 = E, \\ \gamma_1\gamma_2 = C, & \alpha_1\gamma_2 + \alpha_2\gamma_1 = F, & \gamma_1\delta_2 + \gamma_2\delta_1 = K, \\ \delta_1\delta_2 = L, & \alpha_1\delta_2 + \alpha_2\delta_1 = G, & \end{array}$$

and seek to eliminate $\alpha_1, \beta_1, \gamma_1, \delta_1\ \alpha_2, \beta_2, \gamma_2, \delta_2$; or, what is the same, to eliminate $\alpha_1, \beta_1, \gamma_1, \delta_1, \alpha_2$ from

$$\begin{array}{ll} \alpha_1\dfrac{B}{\beta_1} + \alpha_2\beta_1 = D, & \\ \alpha_1\dfrac{C}{\gamma_1} + \alpha_2\gamma_1 = F, & C\beta_1{}^2 + B\gamma_1{}^2 = E\beta_1\gamma_1, \\ & L\gamma_1{}^2 + C\delta_1{}^2 = K\gamma_1\delta_1; \\ \alpha_1\dfrac{L}{\delta_1} + \alpha_2\delta_1 = G, & \end{array}$$

or, what is still the same, to eliminate β, γ, δ, from the triad

$$\left.\begin{array}{r} FL\beta^2\gamma - DL\gamma^2\beta + GB\gamma^2\delta - FB\delta^2\gamma + DC\delta^2\beta - GC\beta^2\delta = 0 \\ C\beta^2 - E\beta\gamma \quad + B\gamma^2 \quad = 0 \\ L\gamma^2 - K\gamma\delta \quad + C\delta^2 \quad = 0 \end{array}\right\}.$$

It is not readily apparent how this would be accomplished in a direct manner, yet we know that by the elimination of H from the two equations

$$\begin{vmatrix} 2B & E & H \\ E & 2C & K \\ H & K & 2L \end{vmatrix} = 0,$$

$$\begin{vmatrix} D & 2B & H \\ F & E & K \\ G & H & 2L \end{vmatrix} = 0,$$

the same resultant will be obtained; and the latter equations, being quadratics in H, the resultant is immediately found to be

$$\begin{vmatrix} C & LE^2 + BK^2 \\ F & 2BGK + 2DEL \end{vmatrix}^2 - \begin{vmatrix} C & EK \\ F & GE + DK \end{vmatrix} \cdot \begin{vmatrix} EK & LE^2 + BK^2 - 4BCL \\ GE + DK & 2BGK + 2DEL - 4BFL \end{vmatrix} = 0.$$

The solution thus suggested is to establish the pair of equations

$$\begin{vmatrix} 2B & E & \theta \\ E & 2C & K \\ \theta & K & 2L \end{vmatrix} = 0 ,$$

$$\begin{vmatrix} D & 2B & \theta \\ F & E & K \\ G & \theta & 2L \end{vmatrix} = 0 ,$$

from the triad in β, γ, δ. This is readily done. To verify the first of the pair we have only got to substitute in it the values of E and K got from the 2nd and 3rd of the triad. To verify the second of the pair we do the same, when it is found that the result is obtainable from the first of the triad by dividing by $\beta\gamma\delta$, and then multiplying by $L\frac{\beta}{\delta} - B\frac{\delta}{\beta}$. The first of the pair is thus seen to be derivable from the last two of the triad, and the second from all three.

As a step towards a direct mode of elimination, it may be noted that the equations are transformable into

$$\left.\begin{array}{l} FL\beta^2 - FB\delta^2 + 2GB\gamma\delta + (DK - GE)\delta\beta - 2DL\beta\gamma = 0 \\ C\beta^2 + B\gamma^2 - E\beta\gamma = 0 \\ L\gamma^2 + C\delta^2 - K\gamma\delta = 0 \end{array}\right\} ,$$

that is to say, into a set of ternary quadrics.

On the Expression of any Bordered Skew Determinant as a Sum of Products of Pfaffians. By Thomas Muir, LL.D.

(Read December 21, 1896.)

1. Cayley commences his third paper on Skew Determinants * (May, 1854) by recalling his development of them in terms of Pfaffians, and then goes on to say :—

" J'ai trouvé récemment une formule analogue pour le développement d'un déterminant gauche bordé, tel que

$$\overline{\alpha 1234 \,\big|\, \beta 1234} = \begin{vmatrix} \alpha\beta, & \alpha 1, & \alpha 2, & \alpha 3, & \alpha 4 \\ 1\beta, & 11, & 12, & 13, & 14 \\ 2\beta, & 21, & 22, & 23, & 24 \\ 3\beta, & 31, & 32, & 33, & 34 \\ 4\beta, & 41, & 42, & 43, & 44 \end{vmatrix} .$$

Cette formule est :

$$\begin{aligned} \overline{\alpha 1234 \,\big|\, \beta 1234} = {} & \alpha\beta 11 \,.\, 22 \,.\, 33 \,.\, 44 \\ & + \alpha\beta 12 \,.\, 12 \,.\, 33 \,.\, 44 \\ & + \alpha\beta 13 \,.\, 13 \,.\, 22 \,.\, 44 \\ & + \alpha\beta 14 \,.\, 14 \,.\, 22 \,.\, 33 \\ & + \alpha\beta 23 \,.\, 23 \,.\, 11 \,.\, 44 \\ & + \alpha\beta 24 \,.\, 24 \,.\, 11 \,.\, 33 \\ & + \alpha\beta 34 \,.\, 34 \,.\, 11 \,.\, 22 \\ & + \alpha\beta 1234 \,.\, 1234 \\ & + \alpha 1 \,.\, \beta 1 \,.\, 22 \,.\, 33 \,.\, 44 \\ & + \alpha 2 \,.\, \beta 2 \,.\, 11 \,.\, 33 \,.\, 44 \\ & + \alpha 3 \,.\, \beta 3 \,.\, 11 \,.\, 22 \,.\, 44 \\ & + \alpha 4 \,.\, \beta 4 \,.\, 11 \,.\, 22 \,.\, 33 \\ & + \alpha 123 \,.\, \beta 123 \,.\, 44 \\ & + \alpha 124 \,.\, \beta 124 \,.\, 33 \\ & + \alpha 134 \,.\, \beta 134 \,.\, 22 \\ & + \alpha 234 \,.\, \beta 234 \,.\, 11 \,. \text{"} \end{aligned}$$

* Cayley, A., "Recherches ultérieures sur les déterminants gauches," *Crelle's Journ.*, l. pp. 299–213 ; or *Collected Math. Papers*, ii. pp. 202–215.

and he explains that the expressions 12, 1234, etc., are Pfaffians, whose law of formation is—

$$12 = 12\,,$$
$$1234 = 12 \cdot 34 + 13 \cdot 42 + 14 \cdot 23\,,$$
$$\begin{aligned}123456 = {} & 12 \cdot 34 \cdot 56 + 13 \cdot 45 \cdot 62 + 14 \cdot 56 \cdot 23 + 15 \cdot 62 \cdot 34 + 16 \cdot 23 \cdot 45 \\ & + 12 \cdot 35 \cdot 64 + 13 \cdot 46 \cdot 25 + 14 \cdot 52 \cdot 36 + 15 \cdot 63 \cdot 42 + 16 \cdot 24 \cdot 53 \\ & + 12 \cdot 36 \cdot 45 + 13 \cdot 42 \cdot 56 + 14 \cdot 53 \cdot 62 + 15 \cdot 64 \cdot 23 + 16 \cdot 25 \cdot 34\,.\end{aligned}$$

No proof is given, and the law of formation of the development itself is not explained.

Rather more than three years afterwards (Nov. 1857)* he returned to the theorem, stating it then by means of two instances, as follows:—

$$\overline{\alpha 123 \;\big|\; \beta 123} = \begin{aligned}[t] & \alpha\beta \,.\, 11 \,.\, 22 \,.\, 33 \\ & + \alpha\beta \,.\, 12 \,.\, 12 \,.\, 33 \\ & + \alpha\beta \,.\, 13 \,.\, 13 \,.\, 22 \\ & + \alpha\beta \,.\, 23 \,.\, 23 \,.\, 11 \\ & + \alpha 1 \,.\, \beta 1 \,.\, 22 \,.\, 33 \\ & + \alpha 2 \,.\, \beta 2 \,.\, 11 \,.\, 33 \\ & + \alpha 3 \,.\, \beta 3 \,.\, 11 \,.\, 22 \\ & + \alpha 123 \,.\, \beta 123\,;\end{aligned}$$

$$\overline{\alpha 1234 \;\big|\; \beta 1234} = \begin{aligned}[t] & \alpha\beta \,.\, 11 \,.\, 22 \,.\, 33 \,.\, 44 \\ & + \alpha\beta \,.\, 12 \,.\, 12 \,.\, 33 \,.\, 44 \\ & + \ldots\ldots\ldots\ldots \\ & + \alpha\beta 1234 \,.\, 1234 \\ & + \ldots\ldots\end{aligned}$$

Manifestly the two statements regarding the determinant of the 5th order do not agree; and, as again no proof is offered, there is no immediate means of ascertaining the correct statement. On turning to the *Collected Mathematical Papers* (vol. ii. p. 203), the confusion is worse confounded, for there neither of the original statements is followed, the theorem being given in a third form, viz.,

$$\overline{\alpha 1234 \;\big|\; \beta 1234} = \begin{aligned}[t] & \alpha\beta \,.\, 11 \,.\, 22 \,.\, 33 \,.\, 44 \\ & + \alpha\beta \,.\, 12 \,.\, 12 \,.\, 33 \,.\, 44 \\ & + \ldots\ldots\ldots\ldots \\ & + \alpha\beta \,.\, 1234 \,.\, 1234 \\ & + \ldots\ldots\ldots\ldots\end{aligned}$$

* Cayley, A., "Théorème sur les déterminants gauches," *Crelle's Journ.*, lv. pp. 277, 278; or *Collected Math. Papers*, iv. pp. 72, 73.

The statement of the identity for the case of the determinant of the 4th order ought to be a help in ascertaining the law of the development, but such is not the case. With the two identities before him, no reader would find it possible, without investigation, to write out the identity for the case of a determinant of higher order.

So far as I am aware, there is no other literature on the subject, which seems indeed to have been entirely lost sight of by mathematicians, although a very special case of the theorem has received considerable attention.

Having recently been led by a straightforward process of deduction to what turns out to be the general theorem which includes the two instances intended to be stated by Cayley, I am in a position, not only to remove the uncertainty above referred to, but also to supply the much-needed proof.

2. First of all, I recall the very special case which has already been proved by several investigators,* and which is best known in the form:—*Every bordered zero-axial skew determinant is expressible as the product of two Pfaffians.* This is necessary as a lemma to what follows. The mode of formation of the two Pfaffians is readily apparent from two examples.

(*a*) Order *even*—

$$\begin{vmatrix} . & a & b & c & d & e \\ -\alpha & . & f & g & h & i \\ -\beta & -f & . & j & k & l \\ -\gamma & -g & -j & . & m & n \\ -\delta & -h & -k & -m & . & p \\ -\epsilon & -i & -l & -n & -p & . \end{vmatrix} = \left| \begin{matrix} a & b & c & d & e \\ & f & g & h & i \\ & & j & k & l \\ & & & m & n \\ & & & & p \end{matrix} \right| \cdot \left| \begin{matrix} \alpha & \beta & \gamma & \delta & \epsilon \\ & f & g & h & i \\ & & j & k & l \\ & & & m & n \\ & & & & p \end{matrix} \right| .$$

(*b*) Order *odd*—

$$\begin{vmatrix} e & a & b & c & d \\ -i & . & f & g & h \\ -l & -f & . & j & k \\ -n & -g & -j & . & m \\ -p & -h & -k & -m & . \end{vmatrix} = \left| \begin{matrix} a & b & c & d & e \\ & f & g & h & -i \\ & & j & k & -l \\ & & & m & -n \\ & & & & -p \end{matrix} \right| \cdot \left| \begin{matrix} f & g & h \\ & j & k \\ & & m \end{matrix} \right| ,$$

* *Quart. Journ. of Math.*, xviii. pp. 46–49.

$$\text{or} \quad = \left| \begin{array}{ccccc} e & a & b & c & d \\ & i & l & n & p \\ & & f & g & h \\ & & & j & k \\ & & & & m \end{array} \right| \cdot \left| \begin{array}{ccc} f & g & h \\ & j & k \\ & & m \end{array} \right| .$$

It will be seen that the Pfaffian product in the latter identity corresponds to Cayley's

$$\alpha\beta 1234 \cdot 1234 ,$$

the definition in the one case being

$$\left| \begin{array}{ccc} f & g & h \\ & j & k \\ & & m \end{array} \right| \equiv fm \quad - \quad gk \quad + \quad hj ,$$

and in the other

$$1234 = 12 \cdot 34 + 13 \cdot 42 + 14 \cdot 23 .$$

3. In the next place I recall the theorem regarding the development of a determinant in terms of co-axial minors having zero elements in the diagonal; that is to say, the theorem of which the following is an instance—

$$|a_1 b_2 c_3 d_4| = \left| \begin{array}{cccc} . & a_2 & a_3 & a_4 \\ b_1 & . & b_3 & b_4 \\ c_1 & c_2 & . & c_4 \\ d_1 & d_2 & d_3 & . \end{array} \right| + \Sigma a_1 \left| \begin{array}{ccc} . & b_3 & b_4 \\ c_2 & . & c_4 \\ d_2 & d_3 & . \end{array} \right| + \Sigma a_1 b_2 \left| \begin{array}{cc} . & c_4 \\ d_3 & . \end{array} \right| + a_1 b_2 c_3 d_4 .$$

For the purpose in view a theorem analogous to this needs to be established; but, as both are cases of a widely general theorem, it is better to prove all the cases at once.

4. Consider any determinant, say the determinant $|a_1 b_2 c_3 d_4 e_5|$, and fix the attention on any number of the diagonal elements, say the elements c_3, d_4, e_5. It is clear that the terms of the final development of the determinant may be separated into four groups, viz.,

1. those containing *all three* of these elements
2. . . . . . *only two* . . . .
3. . . . . . *only one* . . . .
4. . . . . . *none* . . . .

But the terms containing all three are known to be

$$c_3d_4e_5 \begin{vmatrix} a_1 & a_2 \\ b_1 & b_2 \end{vmatrix}.$$

The terms containing only two are those containing only c_3d_4, c_3e_5 or d_4e_5; and as *all* those containing c_3d_4, c_3e_5, d_4e_5 are respectively

$$c_3d_4|\,a_1b_2e_5\,|, \quad c_3e_5|\,a_1b_2d_4\,|, \quad d_4e_5|\,a_1b_2c_3\,|,$$

those containing *only* c_3d_4, c_3e_5, d_4e_5 are respectively

$$c_3d_4 \begin{vmatrix} a_1 & a_2 & a_5 \\ b_1 & b_2 & b_5 \\ e_1 & e_2 & . \end{vmatrix}, \quad c_3e_5 \begin{vmatrix} a_1 & a_2 & a_4 \\ b_1 & b_2 & b_4 \\ d_1 & d_2 & . \end{vmatrix}, \quad d_4e_5 \begin{vmatrix} a_1 & a_2 & a_3 \\ b_1 & b_2 & b_3 \\ c_1 & c_2 & . \end{vmatrix}.$$

Similarly, the terms containing *only one* are those containing only c_3, d_4 or e_5; and as *all* those containing c_3, d_4 or e_5 are respectively

$$c_3\,|\,a_1b_2d_4e_5\,|, \quad d_4\,|\,a_1b_2c_3e_5\,|, \quad e_5\,|\,a_1b_2c_3d_4\,|,$$

those containing *only* c_3, d_4, e_5 are respectively

$$c_3 \begin{vmatrix} a_1 & a_2 & a_4 & a_5 \\ b_1 & b_2 & b_4 & b_5 \\ d_1 & d_2 & . & d_5 \\ e_1 & e_2 & e_4 & . \end{vmatrix}, \quad d_4 \begin{vmatrix} a_1 & a_2 & a_3 & a_5 \\ b_1 & b_2 & b_3 & b_5 \\ c_1 & c_2 & . & c_5 \\ e_1 & e_2 & e_3 & . \end{vmatrix}, \quad e_5 \begin{vmatrix} a_1 & a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 & b_4 \\ c_1 & c_2 & . & c_4 \\ d_1 & d_2 & d_3 & . \end{vmatrix}.$$

Lastly, the terms containing none of the three selected elements are

$$\begin{vmatrix} a_1 & a_2 & a_3 & a_4 & a_5 \\ b_1 & b_2 & b_3 & b_4 & b_5 \\ c_1 & c_2 & . & c_4 & c_5 \\ d_1 & d_2 & d_3 & . & d_5 \\ e_1 & e_2 & e_3 & e_4 & . \end{vmatrix}.$$

Consequently we have the identity—

$$|\,a_1b_2c_3d_4e_5\,| = \begin{vmatrix} a_1 & a_2 & a_3 & a_4 & a_5 \\ b_1 & b_2 & b_3 & b_4 & b_5 \\ c_1 & c_2 & . & c_4 & c_5 \\ d_1 & d_2 & d_3 & . & d_5 \\ e_1 & e_2 & e_3 & e_4 & . \end{vmatrix} + \Sigma c_3 \begin{vmatrix} a_1 & a_2 & a_4 & a_5 \\ b_1 & b_2 & b_4 & b_5 \\ d_1 & d_2 & . & d_5 \\ e_1 & e_2 & e_4 & \end{vmatrix}$$

$$+ \Sigma c_3d_4 \begin{vmatrix} a_1 & a_2 & a_5 \\ b_1 & b_2 & b_5 \\ e_1 & e_2 & . \end{vmatrix} + c_3d_4e_5 \begin{vmatrix} a_1 & a_2 \\ b_1 & b_2 \end{vmatrix},$$

the mode of formation of the right-hand member being (1) to take the various combinations of c_3, d_4, e_5, viz.,

$$1;\quad c_3,\ d_4,\ e_5;\quad c_3d_4,\ c_3e_5,\ d_4e_5;\quad c_3d_4e_5;$$

(2) to take as co-factors of these their co-factors in the determinant, only replacing in the latter the elements c_3, d_4, e_5 by 0 in every case.

The number of terms in the development is thus seen to be

$$1 + C_{3,1} + C_{3,2} + C_{3,3}$$

i.e.,

$$(1+1)^3 .$$

5. It is very interesting to observe the relation of the various cases of this general theorem to one another; it is useful also, as a knowledge of it serves to suggest another mode of proof. Thus, taking the simplest possible case—that referred to in § 3 as being hitherto well known—viz.,

$$|a_1 b_2 c_3 d_4 e_5| = \begin{vmatrix} \cdot & a_2 & a_3 & a_4 & a_5 \\ b_1 & \cdot & b_3 & b_4 & b_5 \\ c_1 & c_2 & \cdot & c_4 & c_5 \\ d_1 & d_2 & d_3 & \cdot & d_5 \\ e_1 & e_2 & e_3 & e_4 & \cdot \end{vmatrix} + \Sigma a_1 \begin{vmatrix} \cdot & b_3 & b_4 & b_5 \\ c_2 & \cdot & c_4 & c_5 \\ d_2 & d_3 & \cdot & d_5 \\ e_2 & e_3 & e_4 & \cdot \end{vmatrix}$$

$$+ \Sigma a_1 b_2 \begin{vmatrix} \cdot & c_4 & c_5 \\ d_3 & \cdot & d_5 \\ e_3 & e_4 & \cdot \end{vmatrix} + \Sigma a_1 b_2 c_3 \begin{vmatrix} \cdot & d_5 \\ e_4 & \cdot \end{vmatrix} + a_1 b_2 c_3 d_4 e_5, \qquad (E_1)$$

and adding the first of the $C_{5,1}$ terms to the term before it, the first four of the $C_{5,2}$ terms to the remaining four of the $C_{5,1}$ terms, and the first six of the $C_{5,3}$ terms to the remaining six of the $C_{5,2}$ terms we have

$$|a_1 b_2 c_3 d_4 e_5| = \begin{vmatrix} a_1 & a_2 & a_3 & a_4 & a_5 \\ b_1 & \cdot & b_3 & b_4 & b_5 \\ c_1 & c_2 & \cdot & c_4 & c_5 \\ d_1 & d_2 & d_3 & \cdot & d_5 \\ e_1 & e_2 & e_3 & e_4 & \cdot \end{vmatrix} + \Sigma b_2 \begin{vmatrix} a_1 & a_3 & a_4 & a_5 \\ c_1 & \cdot & c_4 & c_5 \\ d_1 & d_2 & \cdot & d_5 \\ e_1 & e_2 & e_4 & \cdot \end{vmatrix}$$

$$+ \Sigma b_2 c_3 \begin{vmatrix} a_1 & a_4 & a_5 \\ d_1 & \cdot & d_5 \\ e_1 & e_4 & \cdot \end{vmatrix} + \Sigma b_2 c_3 d_4 \begin{vmatrix} a_1 & a_5 \\ e_1 & \cdot \end{vmatrix} + b_2 c_3 d_4 e_5 a_1 \qquad (E_2)$$

which is the next case of the theorem, viz., the case where the diagonal elements of the determinants in the development are all 0 *except one.*

Again, adding the first of the $C_{4,1}$ terms to the term before it, the first three of the $C_{4,2}$ terms to the remaining three of the $C_{4,1}$ terms, the first three of the $C_{4,3}$ to the remaining three of the $C_{4,2}$ terms, and the last term to the remaining one of the $C_{4,3}$ terms, we obtain the next case of the theorem, viz., that used in § 4.

Treating the expansion now reached in the same way, we find the next case, viz.,

$$|a_1b_2c_3d_4e_5 = \begin{vmatrix} a_1 & a_2 & a_3 & a_4 & a_5 \\ b_1 & b_2 & b_3 & b_4 & b_5 \\ c_1 & c_2 & c_3 & c_4 & c_5 \\ d_1 & d_2 & d_3 & . & d_5 \\ e_1 & e_2 & e_3 & e_4 & . \end{vmatrix} + \sum d_4 \begin{vmatrix} a_1 & a_2 & a_3 & a_5 \\ b_1 & b_2 & b_3 & b_5 \\ c_1 & c_2 & c_3 & c_5 \\ e_1 & e_2 & e_3 & . \end{vmatrix} + d_4e_5 \begin{vmatrix} a_1 & a_2 & a_3 \\ b_1 & b_2 & b_3 \\ c_1 & c_2 & c_3 \end{vmatrix}; \qquad (E_4)$$

and thence, in the same way,

$$|a_1b_2c_3d_4e_5| = \begin{vmatrix} a_1 & a_2 & a_3 & a_4 & a_5 \\ b_1 & b_2 & b_3 & b_4 & b_5 \\ c_1 & c_2 & c_3 & c_4 & c_5 \\ d_1 & d_2 & d_3 & d_4 & d_5 \\ e_1 & e_2 & e_3 & e_4 & . \end{vmatrix} + e_5 \begin{vmatrix} a_1 & a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 & b_4 \\ c_1 & c_2 & c_3 & c_4 \\ d_1 & d_2 & d_3 & d_4 \end{vmatrix}; \qquad (E_5)$$

and, lastly, of course,

$$|a_1b_2c_3d_4e_5| = a_1b_2c_3d_4e_5| .$$

6. The reverse process is equally interesting. Starting with $|a_1b_2c_3d_4e_5|$ we separate its terms into two groups, viz.,

(1) those which contain e_5,
(2) those which do not,

and thus obtain (E_5).

Then we take each of the two groups of (E_5) and separate it into two groups, viz.,

(1) those which contain d_4,
(2) those which do not.

In this way we obtain the four groups of (E_4).

Next we partition these four into the eight of (E_3), and so on.

When we come to partition the groups of (E_2), we find it impracticable in the case of one group; hence the break in the series which gives the number of terms in the various developments, viz.,

$$2^1,\ 2^2,\ 2^3,\ 2^4,\ 2^5 - C_{5,4}.$$

7. Of these different developments it is the second (E_2) which is useful in connection with the proposed Pfaffian development of a bordered skew determinant.

Let the said determinant be

$$\left|\begin{array}{ccccc} m_1 & h_1 & h_2 & h_3 & h_4 \\ -k_1 & m_2 & \alpha_1 & \alpha_2 & \alpha_3 \\ -k_2 & -\alpha_1 & m_3 & \beta_1 & \beta_2 \\ -k_3 & -\alpha_2 & -\beta_1 & m_4 & \gamma_1 \\ -k_4 & -\alpha_3 & -\beta_2 & -\gamma_1 & m_5 \end{array}\right|$$

and let us at once apply (E_2). The result is

$$\left|\begin{array}{ccccc} m_1 & h_1 & h_2 & h_3 & h_4 \\ -k_1 & \cdot & \alpha_1 & \alpha_2 & \alpha_3 \\ -k_2 & -\alpha_1 & \cdot & \beta_1 & \beta_2 \\ -k_3 & -\alpha_2 & -\beta_1 & \cdot & \gamma_1 \\ -k_4 & -\alpha_3 & -\beta_2 & -\gamma_1 & \cdot \end{array}\right| + \sum m_2 \left|\begin{array}{cccc} m_1 & h_2 & h_3 & h_4 \\ -k_2 & \cdot & \beta_1 & \beta_2 \\ -k_3 & -\beta_1 & \cdot & \gamma_1 \\ -k_4 & -\beta_2 & -\gamma_1 & \cdot \end{array}\right|$$

$$+ \sum m_2 m_3 \left|\begin{array}{ccc} m_1 & h_3 & h_4 \\ -k_3 & \cdot & \gamma_1 \\ -k_4 & -\gamma_1 & \cdot \end{array}\right| + \sum m_2 m_3 m_4 \left|\begin{array}{cc} m_1 & h_4 \\ -k_4 & \cdot \end{array}\right| + m_1 m_2 m_3 m_4 m_5 .$$

But every determinant here is a *bordered zero-axial skew* determinant, and therefore, by Cayley's special case, is expressible as the product of two Pfaffians, the expansion thus becoming (§ 2)

$$\left|\begin{array}{ccc} \alpha_1 & \alpha_2 & \alpha_3 \\ & \beta_1 & \beta_2 \\ & & \gamma_1 \end{array}\right| \cdot \left|\begin{array}{ccccc} m_1 & h_1 & h_2 & h_3 & h_4 \\ & k_1 & k_2 & k_3 & k_4 \\ & & \alpha_1 & \alpha_2 & \alpha_3 \\ & & & \beta_1 & \beta_2 \\ & & & & \gamma_1 \end{array}\right| + \sum m_2 \left|\begin{array}{ccc} h_2 & h_3 & h_4 \\ & \beta_1 & \beta_2 \\ & & \gamma_1 \end{array}\right| \cdot \left|\begin{array}{ccc} k_2 & k_3 & k_4 \\ & \beta_1 & \beta_2 \\ & & \gamma_1 \end{array}\right|$$

$$+ \sum m_2 m_3 \cdot \gamma_1 \left|\begin{array}{ccc} m_1 & h_3 & h_4 \\ & k_3 & k_4 \\ & & \gamma_1 \end{array}\right| + \sum m_2 m_3 m_4 \cdot h_4 k_4 + m_1 m_2 m_3 m_4 m_5 .$$

The corresponding identity for the case of the 4th order evidently is—

$$\begin{vmatrix} m_1 & h_1 & h_2 & h_3 \\ -k_1 & m_2 & \alpha_1 & \alpha_2 \\ -k_2 & -\alpha_1 & m_3 & \beta_1 \\ -k_3 & -\alpha_2 & -\beta_1 & m_4 \end{vmatrix} = \left| \begin{array}{ccc} h_1 & h_2 & h_3 \\ & \alpha_1 & \alpha_2 \\ & & \beta_1 \end{array} \right| \cdot \left| \begin{array}{ccc} k_1 & k_2 & k_2 \\ & \alpha_1 & \alpha_2 \\ & & \beta_1 \end{array} \right|$$

$$+ m_2\beta_1 \left| \begin{array}{ccc} m_1 & h_2 & h_3 \\ & k_2 & k_3 \\ & & \beta_1 \end{array} \right| + m_3\alpha_2 \left| \begin{array}{ccc} m_1 & h_1 & h_3 \\ & k_1 & k_3 \\ & & \alpha_2 \end{array} \right| + m_4\alpha_1 \left| \begin{array}{ccc} m_1 & h_1 & h_2 \\ & k_1 & k_2 \\ & & \alpha_1 \end{array} \right|$$

$$+ m_2 m_3 h_3 k_3 + m_2 m_4 h_2 k_2 + m_3 m_4 h_1 k_1$$

$$+ m_1 m_2 m_3 m_4 ,$$

as also may readily be seen on putting

$$h_4 = \alpha_3 = \beta_2 = \gamma_1 = 0 , \quad m_5 = 1$$

in the identity for the 5th order.

8. Translating into Cayley's notation, we find these must be written

$$\overline{\alpha 1234 \,\big|\, \beta 1234} = 1234 \cdot \alpha\beta 1234 + \sum 11 \cdot \alpha 234 \cdot \beta 234$$

$$+ \sum 11 \cdot 22 \cdot 34 \cdot \alpha\beta 34 + \sum 11 \cdot 22 \cdot 33 \cdot \alpha 4 \cdot \beta 4$$

$$+ \alpha\beta \cdot 11 \cdot 22 \cdot 33 \cdot 44 ;$$

and

$$\overline{\alpha 123 \,\big|\, \beta 123} = \alpha 123 \cdot \beta 123$$

$$+ \sum 11 \cdot 23 \cdot \alpha\beta 23 + \sum 11 \cdot 22 \cdot \alpha 3 \cdot \beta 3$$

$$+ \alpha\beta \cdot 11 \cdot 22 \cdot 33 .$$

The former identity is thus readily seen to be given incorrectly in *Crelle's Journ.* lv. p. 277, and *Collected Math. Papers*, iv. p. 73; still more incorrectly—indeed, ridiculously so—in *Collected Math. Papers*, ii. p. 203; and almost correctly in *Crelle's Journ.* l. p. 300, although even here $\alpha\beta 11$ is put instead of $\alpha\beta \cdot 11$, to which single term it is fortunately equal, as the two other terms cancel each other.

The other identity is quite incorrectly given both in the original and in the reprint.

9. In the skew determinant which he borders, Cayley uses diagonal elements which are all different, and the same course has been followed here for the purpose of a perfectly satisfactory comparison of the results. It is most important to observe, however, that there is no great gain in generality in doing this in preference to using diagonal elements which are all unity; for, so long as the diagonal elements are not zero, any one of them, m say, can be changed into 1, without the determinant ceasing to be skew, by dividing the elements of the row and of the column in which it occurs by $\sqrt{m}$; thus—

$$\begin{vmatrix} m & \alpha & \beta \\ -\alpha & n & \gamma \\ -\beta & -\gamma & r \end{vmatrix} = mnr \begin{vmatrix} 1 & \dfrac{\alpha}{\sqrt{mn}} & \dfrac{\beta}{\sqrt{mr}} \\ -\dfrac{\alpha}{\sqrt{mn}} & 1 & \dfrac{\gamma}{\sqrt{nr}} \\ -\dfrac{\beta}{\sqrt{mr}} & -\dfrac{\gamma}{\sqrt{nr}} & 1 \end{vmatrix} .$$

And as unit-axial skew determinants are those which occur in connection with orthogonal substitution,—the most important sphere for the application of skew determinants,—it is very desirable to employ this seemingly special kind in the formal statement of theorems.

Doing this we write, for the purpose of making clear the law of development, the first four cases of our theorem, as follows:—

$$\begin{vmatrix} m & h_1 & h_2 \\ -k_1 & 1 & \alpha_1 \\ -k_2 & -\alpha_1 & 1 \end{vmatrix} = m + (h_1k_1 + h_2k_2) + \alpha_1 \left|\begin{array}{ccc} m & h_1 & h_2 \\ & k_1 & k_2 \\ & & \alpha_1 \end{array}\right| ,$$

$$\begin{vmatrix} m & h_1 & h_2 & h_3 \\ -k_1 & 1 & \alpha_1 & \alpha_2 \\ -k^2 & -\alpha_1 & 1 & \beta_1 \\ -k_3 & -\alpha_2 & -\beta_1 & 1 \end{vmatrix} = m + (h_1k_1 + h_2k_2 + h_3k_3)$$
$$+ \alpha_1 \left|\begin{array}{ccc} m & h_1 & h_2 \\ & k_1 & k_2 \\ & & \alpha_1 \end{array}\right| + \alpha_2 \left|\begin{array}{ccc} m & h_1 & h_3 \\ & k_1 & k_3 \\ & & \alpha^2 \end{array}\right| + \beta_1 \left|\begin{array}{ccc} m & h_2 & h_3 \\ & k_2 & k_3 \\ & & \beta_1 \end{array}\right|$$
$$+ \left|\begin{array}{ccc} h_1 & h_2 & h_3 \\ & \alpha_1 & \alpha_2 \\ & & \beta_1 \end{array}\right| \cdot \left|\begin{array}{ccc} k_1 & k_2 & k_3 \\ & \alpha_1 & \alpha_2 \\ & & \beta_1 \end{array}\right| ,$$

$$\begin{vmatrix} m & h_1 & h_2 & h_3 & h_4 \\ -k_1 & 1 & \alpha_1 & \alpha_2 & \alpha_3 \\ -k_2 & -\alpha_1 & 1 & \beta_1 & \beta_2 \\ -k_3 & -\alpha_2 & -\beta_1 & 1 & \gamma_1 \\ -k_4 & -\alpha_3 & -\beta_2 & -\gamma_1 & 1 \end{vmatrix} = m + (h_1k_1 + h_2k_2 + h_3k_3 + h_4k_4)$$

$$+ \Sigma\alpha_1 \left| \begin{array}{ccc} m & h_1 & h_2 \\ & k_1 & k_2 \\ & & \alpha_1 \end{array} \right| \text{(6 terms)}$$

$$+ \Sigma \left| \begin{array}{ccc} h_1 & h_2 & h_3 \\ & \alpha_1 & \alpha_2 \\ & & \beta_1 \end{array} \right| . \left| \begin{array}{ccc} k_1 & k_2 & k_3 \\ & \alpha_1 & \alpha_2 \\ & & \beta_1 \end{array} \right| \text{(4 terms)}$$

$$+ \left| \begin{array}{ccc} \alpha_1 & \alpha_2 & \alpha_3 \\ & \beta_1 & \beta_2 \\ & & \gamma_1 \end{array} \right| . \left| \begin{array}{ccccc} m & h_1 & h_2 & h_3 & h_4 \\ & k_1 & k_2 & k_3 & k_4 \\ & & \alpha_1 & \alpha_2 & \alpha_3 \\ & & & \beta_1 & \beta_2 \\ & & & & \gamma_1 \end{array} \right| ,$$

$$\begin{vmatrix} m & h_1 & h_2 & h_3 & h_4 & h_5 \\ -k_1 & 1 & \alpha_1 & \alpha_2 & \alpha_3 & \alpha_4 \\ -k_2 & -\alpha_1 & 1 & \beta_1 & \beta_2 & \beta_3 \\ -k_3 & -\alpha_2 & -\beta_1 & 1 & \gamma_1 & \gamma_2 \\ -k_4 & -\alpha_3 & -\beta_2 & -\gamma_1 & 1 & \delta_1 \\ -k_5 & -\alpha_4 & -\beta_3 & -\gamma_2 & -\delta_1 & 1 \end{vmatrix} = m + (h_1k_1 + h_2k_2 + \ldots .)$$

$$+ \Sigma\alpha_1 \left| \begin{array}{ccc} m & h_1 & h_2 \\ & k_1 & k_2 \\ & & \alpha_1 \end{array} \right| \text{(10 terms)}$$

$$+ \Sigma \left| \begin{array}{ccc} h_1 & h_2 & h_3 \\ & \alpha_1 & \alpha_2 \\ & & \beta_1 \end{array} \right| \cdot \left| \begin{array}{ccc} k_1 & k_2 & k_3 \\ & \alpha_1 & \alpha_2 \\ & & \beta_1 \end{array} \right| \text{(10 terms)}$$

$$+ \Sigma \left| \begin{array}{ccc} \alpha_1 & \alpha_2 & \alpha_3 \\ & \beta_1 & \beta_2 \\ & & \gamma_1 \end{array} \right| \cdot \left| \begin{array}{ccccc} m & h_1 & h_2 & h_3 & h_4 \\ & k_1 & k_2 & k_3 & k_4 \\ & & \alpha_1 & \alpha_2 & \alpha_3 \\ & & & \beta_1 & \beta_2 \\ & & & & \gamma_1 \end{array} \right| \text{(5 terms)}$$

$$+ \left| \begin{array}{ccccc} h_1 & h_2 & h_3 & h_4 & h_5 \\ & \alpha_1 & \alpha_2 & \alpha_3 & \alpha_4 \\ & & \beta_1 & \beta_2 & \beta_3 \\ & & & \gamma_1 & \gamma_2 \\ & & & & \delta_1 \end{array} \right| \cdot \left| \begin{array}{ccccc} k_1 & k_2 & k_3 & k_4 & k_5 \\ & \alpha_1 & \alpha_2 & \alpha_3 & \alpha_4 \\ & & \beta_1 & \beta_2 & \beta_3 \\ & & & \gamma_1 & \gamma_2 \\ & & & & \delta_1 \end{array} \right| .$$

Looking at the last of the four we observe—

(1) that the 1st and 3rd groups of Pfaffian products resemble each other, as do also the 2nd and 4th.

(2) that in the 1st and 3rd groups of Pfaffian products the one Pfaffian is a minor of the other,—is, in fact, the co-factor of m in that other.

(3) that in the 1st and 3rd groups of Pfaffian products the first

line of the Pfaffian of higher order is got by taking m and an even number of h's.

(4) that in the 2nd and 4th groups of Pfaffian products the Pfaffians are of like order, the second Pfaffian differing from the first merely in having k's for h's.

(5) that in the 2nd and 4th groups of Pfaffian products the first line of the first Pfaffian is got by taking an odd number of h's.

Bearing these facts in mind, there will be little difficulty in writing out the development in any case.

10. Knowing that the number of terms in the final expansion of a Pfaffian of the 1st, 2nd, 3rd, ... orders are respectively 1, 3, 3·5, 3·5·7, ... we see that the number of terms in a bordered skew determinant, as given by the preceding development, is

for the 3rd order,	$1+2\cdot1+1\cdot3$	*i.e.*	6,
„ 4th „	$1+3\cdot1+3\cdot3+1\cdot3^2$	*i.e.*	22,
„ 5th „	$1+4\cdot1+6\cdot3+4\cdot3^2+1\cdot3^2\cdot5$	*i.e.*	104,
„ 6th „	$1+5\cdot1+10\cdot3+10\cdot3^2+5\cdot3^2\cdot5+1\cdot3^2\cdot5^2$	*i.e.*	576,
„ 7th „	$1+6\cdot1+15\cdot3+20\cdot3^2+15\cdot3^2\cdot5+6\cdot3^2\cdot5^2+1\cdot3^2\cdot5^2\cdot7^2$	*i.e.*	3832,

. . . .

so that the number of terms of a bordered skew determinant which cancel each other, and which we, by using the said development, are saved considering, are

for the 3rd order	0
„ 4th „	2
„ 5th „	16
„ 6th „	144
„ 7th „	1208

. . . . . .

11. In connection with this, however, it has to be noted that we must not use the numbers 6, 22, ... as being the actual numbers of *unlike* terms in the determinants in question, because, when we come to determinants of the 5th order, the terms of the expansion of $(\alpha_1\gamma_1 - \alpha_2\beta_2 + \alpha_3\beta_1)^2$ make their appearance in the result, and these have to be counted as being 6 in number instead

of 9. Similarly, when we come to the 7th order the square of a 15-termed expression turns up, and this we must now count as containing, not 15^2 terms, but $15^2 - C_{15,2}$. The numbers of unlike terms are thus—

for the 5th order $1+4\cdot1+6\cdot3+4\cdot3^2+(3^2\cdot5-3) = 101$,
,, 6th ,, $1+5\cdot1+10\cdot3+10\cdot3^2+5(3^2\cdot5-3)+(15^2-5\cdot3) = 546$,
,, 7th ,, $1+6\cdot1+15\cdot3+20\cdot3^2+15(3^2\cdot5-3)+6(15^2-5\cdot3)+(15^2\cdot7-105-6\cdot5\cdot3)=3502$,

. . . .

12. As a bordered skew determinant of the nth order can be expressed as a sum of $n-1$ such determinants of the $(n-1)$th order, together with a skew determinant of the latter order, and as the number of unlike terms in a skew determinant is known,* it is clear that we have a ready means of verifying the figures just obtained.

Denoting the number of unlike terms in a skew determinant of the nth order by S_n we have *

$$S_3=4, \quad S_4=13, \quad S_5=41, \quad S_6=226, \quad S_7=1072, \quad \ldots$$

so that, if $(BS)_n$ be used in a similar way in connection with bordered skew determinants, we have

$$\begin{aligned}
(BS)_4 &= S_3+3(BS)_3 = 4+3\cdot6 = 22,\\
(BS)_5 &= S_4+4(BS)_4 = 13+4\cdot22 = 101,\\
(BS)_6 &= S_5+5(BS)_5 = 41+5\cdot101 = 546,\\
(BS)_7 &= S_6+6(BS)_6 = 226+6\cdot546 = 3502,\\
&\ldots\ldots\ldots\ldots
\end{aligned}$$

exactly as in the preceding section.

It should also be noted that as a consequence of this

$$(BS)_8 = S_7+7S_6+7\cdot6S_5+\ldots\ldots+7\cdot6\cdot5\cdot4\cdot3\cdot2\cdot1\,.$$

13. The most important special case of the theorem is got by putting $k_1, k_2, k_3, \ldots = h_1, h_2, h_3, \ldots$, for then the bordered determinant becomes itself a skew determinant. A notable change takes place also in the development, the two Pfaffians in every product, even where in the general theorem they are of different

* Cunningham, Allan, "An Investigation of the Number of Constituents, Elements, and Minors of a Determinant," *Journ. of Science*, iv. (1874), pp. 212–228.

orders, becoming equal. For example, when the order is the 5th the product

$$\left|\begin{array}{ccc} \alpha_1 & \alpha_2 & \alpha_3 \\ & \beta_1 & \beta_2 \\ & & \gamma_1 \end{array}\right| \cdot \left|\begin{array}{ccccc} m_1 & h_1 & h_2 & h_3 & h_4 \\ & h_1 & h_2 & h_3 & h_4 \\ & & \alpha_1 & \alpha_2 & \alpha_3 \\ & & & \beta_1 & \beta_2 \\ & & & & \gamma_1 \end{array}\right| ,$$

on account of the identity of the first two frame-lines of the second Pfaffian, becomes

$$m_1 \left|\begin{array}{ccc} \alpha_1 & \alpha_2 & \alpha_2 \\ & \beta_1 & \beta_2 \\ & & \gamma_1 \end{array}\right|^2 ;$$

and the full statement of the identity is

$$\left|\begin{array}{ccccc} m_1 & h_1 & h_2 & h_3 & h_4 \\ -h_1 & m_2 & \alpha_1 & \alpha_2 & \alpha_3 \\ -h_2 & -\alpha_1 & m_3 & \beta_1 & \beta_2 \\ -h_3 & -\alpha_2 & -\beta_1 & m_4 & \gamma_1 \\ -h_4 & -\alpha_3 & -\beta_2 & -\gamma_1 & m_5 \end{array}\right| = m_1 \left|\begin{array}{ccc} \alpha_1 & \alpha_2 & \alpha_3 \\ & \beta_1 & \beta_2 \\ & & \gamma_1 \end{array}\right|^2 + \sum m_2 \left|\begin{array}{ccc} h_2 & h_3 & h_4 \\ & \beta_1 & \beta_2 \\ & & \gamma_1 \end{array}\right|^2$$

$$+ m_1 \sum m_2 m_3 \cdot \gamma_1{}^2 + \sum m_2 m_3 m_4 \cdot h_4{}^2 + m_1 m_2 m_3 m_4 m_5 ,$$

or

$$= \sum m_1 \left|\begin{array}{ccc} \alpha_1 & \alpha_2 & \alpha_3 \\ & \beta_1 & \beta_2 \\ & & \gamma_1 \end{array}\right|^2 + \sum m_1 m_2 m_3 \cdot \gamma_1{}^2 + m_1 m_2 m_3 m_4 m_5 ,$$

if we bear in mind the wider sphere to which Σ now refers.

This special case is, as implied in § 1, that first dealt with by Cayley, being the subject of his second paper on Skew Determinants.*

14. A consideration of this expansion of a skew determinant suffices to determine the number of unlike terms in such a determinant. For example, for the 5th order we clearly have the number of unlike terms

$$\begin{aligned} &= C_{5,1}(3^2 - C_{3,1}) + C_{5,3} + C_{5,5} , \\ &= 30 \qquad\qquad\quad + 10 \quad + 1 , \\ &= 41 . \end{aligned}$$

* Cayley, A., "Sur les déterminants gauches," *Crelle's Journ.*, xxxviii. pp. 93-96; or *Collected Math. Papers*, i. pp. 410-413.

And, generally, the number of unlike terms in a skew determinant of the nth order is *

$$1 + C_{n,2} + C_{n,4} \cdot \frac{3(1+3)}{2} + C_{n,6}\frac{3\cdot 5(1+3\cdot 5)}{2} + C_{n,8}\frac{3\cdot 5\cdot 7(1+3\cdot 5\cdot 7)}{2} + \cdots .$$

15. If we specialise further, by making $m_1 = m_2 = m_3 = m_4 = m_5 = 1$, the expansion takes the form of a sum of squares.

If, on the other hand, we make each of the m's equal to 0, the expansion in the case of odd orders entirely disappears; and, in the case of even orders, reduces to one term. This is the "very special case" referred to in §2, and used in §7 in proving the general theorem. It is the subject of Cayley's fourth paper on Skew Determinants.†

It would be well, however, to combine these two special cases and others in one statement, viz., that, *if the values of the diagonal elements of a skew determinant be confined to* 0 *or* 1, *the determinant is expressible as a sum of squares of Pfaffians.* For example, in the case of the 5th order, if $m_1 = 0$, and $m_2 = m_3 = m_4 = m_5 = 1$, we have

$$\begin{vmatrix} \cdot & h_1 & h_2 & h_3 & h_4 \\ -h_1 & 1 & \alpha_1 & \alpha_2 & \alpha_3 \\ -h_2 & -\alpha_1 & 1 & \beta_1 & \beta_2 \\ -h_3 & -\alpha_2 & -\beta_1 & 1 & \gamma_1 \\ -h_4 & -\alpha_3 & -\beta_2 & -\gamma_1 & 1 \end{vmatrix} = \begin{vmatrix} h_2 & h_3 & h_4 \\ & \beta_1 & \beta_2 \\ & & \gamma_1 \end{vmatrix}^2 + \begin{vmatrix} h_1 & h_3 & h_4 \\ & \alpha_2 & \alpha_3 \\ & & \gamma_1 \end{vmatrix}^2 + \begin{vmatrix} h_1 & h_2 & h_4 \\ & \alpha_1 & \alpha_3 \\ & & \beta_2 \end{vmatrix}^2 + \begin{vmatrix} h_1 & h_2 & h_3 \\ & \alpha_1 & \alpha_2 \\ & & \beta_1 \end{vmatrix}^2$$
$$+ h_4^2 + \alpha_3^2 + \beta_2^2 + h_1^2 .$$

16. Before proceeding to our next theorem, the nature and notation of the elements of product-determinants require to be recalled to mind. The elements of the determinant which is the product of $|a_1 b_2 c_3|$ and $|\alpha_1 \beta_2 \gamma_3|$ are of the form,

$$a_1\alpha_1 + a_2\beta_1 + a_3\gamma_1 ,$$
$$\text{or } (a_1 a_2 a_3)(\alpha_1 \beta_1 \gamma_1),$$

* *Cf.* Cunningham's paper, above referred to, p. 225.

† Cayley, A., "Théorème sur les déterminants gauches," *Crelle's Journ.*, lv. pp. 277, 278; or *Collected Math. Papers*, iv. pp. 72, 73.

or, for compactness' sake,

$$\frac{a_1a_2a_3}{\alpha_1\beta_1\gamma_1}.$$

In the next place, the product $|a_1b_2c_3|\cdot|\alpha_1\beta_2\gamma_3|\cdot|x_1y_2z_3|$ has elements of the form

$$x_1\frac{a_1a_2a_3}{\alpha_1\beta_1\gamma_1} + y_1\frac{a_1a_2a_3}{\alpha_2\beta_2\gamma_2} + z_1\frac{a_1a_2a_3}{\alpha_3\beta_3\gamma_3},$$

or

$$\begin{array}{ccc|c} a_1 & a_2 & a_3 & \\ \hline \alpha_1 & \beta_1 & \gamma_1 & x_1 \\ \alpha_2 & \beta_2 & \gamma_2 & y_1 \\ \alpha_3 & \beta_3 & \gamma_3 & z_1 \end{array},$$

the final expansion of the latter expression being easily obtainable on remembering that there is a term corresponding to every element in the square array, and that this term is the product of that element, β_3 say, and the two outside elements a_2 and z_1 standing in the same column or row with it.

Further than these two cases it is not necessary at present to go. The succeeding expressions of the same kind will be found in a paper published in *Transactions* of the Society, where also an exposition of their properties is given.*

17. Now, the first three instances of our new theorem are—

$$m\begin{vmatrix}1 & \alpha_1\\ -\alpha_1 & 1\end{vmatrix} + \left.\begin{array}{cc} h_1 & h_2 \\ \hline 1 & \alpha_1 \\ -\alpha_1 & 1\end{array}\right|\begin{array}{l} \\ k_1 \\ k_2\end{array} = m + \frac{h_1\ h_2}{k_1\ k_2} + \alpha_1\left|\begin{array}{ccc} m & h_1 & h_2 \\ & k_1 & k_2 \\ & & \alpha_1\end{array}\right|,$$

$$m\begin{vmatrix}1 & \alpha_1 & \alpha_2\\ -\alpha_1 & 1 & \beta_1 \\ -\alpha_2 & -\beta_1 & 1\end{vmatrix} + \left.\begin{array}{ccc} h_1 & h_2 & h_3 \\ \hline 1 & \alpha_1 & \alpha_2 \\ -\alpha_1 & 1 & \beta_1 \\ -\alpha_2 & -\beta_1 & 1\end{array}\right|\begin{array}{l} \\ k_1 \\ k_2 \\ k_3\end{array} = m + \frac{h_1h_2h_3}{k_1k_2k_3} + \Sigma\alpha_1\left|\begin{array}{ccc} m & h_1 & h_2 \\ & k_1 & k_2 \\ & & \alpha_1\end{array}\right|,$$

$$m\begin{vmatrix}1 & \alpha_1 & \alpha_2 & \alpha_3\\ -\alpha_1 & 1 & \beta_1 & \beta_2 \\ -\alpha_2 & -\beta_1 & 1 & \gamma_1 \\ -\alpha_3 & -\beta_2 & -\gamma_1 & 1\end{vmatrix} + \left.\begin{array}{cccc} h_1 & h_2 & h_3 & h_4 \\ \hline 1 & \alpha_1 & \alpha_2 & \alpha_3 \\ -\alpha_1 & 1 & \beta_1 & \beta_2 \\ -\alpha_2 & -\beta_1 & 1 & \gamma_1 \\ -\alpha_3 & -\beta_2 & -\gamma_1 & 1\end{array}\right|\begin{array}{l} \\ k_1 \\ k_2 \\ k_3 \\ k_4\end{array} = m + \frac{h_1h_2h_3h_4}{k_1k_2k_3k_4}\Sigma + \alpha_1\left|\begin{array}{ccc} m & h_1 & h_2 \\ & k_1 & k_2 \\ & & \alpha_1\end{array}\right. + m\left|\begin{array}{ccc} \alpha_1 & \alpha_2 & \alpha_3 \\ & \beta_1 & \beta_2 \\ & & \gamma_1\end{array}\right|^2.$$

* "On Bipartite Functions," *Trans. R.S.E.*, xxxii. pp. 461–481.

It will be observed that the left-hand member involves (1) a skew determinant and (2) a bipartite function having the same square array as the determinant, and that the right-hand member closely resembles the Pfaffian development of § 9. To prove the theorem it will suffice to show how the next identity is deduced from the last of the preceding three. For shortness' sake, let us express this last in the form

$$S_4 + B_4 = E_4,$$

then we have the left-hand member of the next identity, viz.,

$$S_5 + B_5 = S_4 + m\begin{vmatrix} 1 & \alpha_1 & \alpha_2 & \alpha_3 & \alpha_4 \\ -\alpha_1 & 1 & \beta_1 & \beta_2 & \beta_3 \\ -\alpha_2 & -\beta_1 & 1 & \gamma_1 & \gamma_2 \\ -\alpha_3 & -\beta_2 & -\gamma_1 & 1 & \delta_1 \\ -\alpha_4 & -\beta_3 & -\gamma_2 & -\delta_1 & \cdot \end{vmatrix} + B_4 + \begin{array}{ccccc|c} h_1 & h_2 & h_3 & h_4 & h_5 & \\ \hline \cdot & \cdot & \cdot & \cdot & \alpha_4 & k_1 \\ \cdot & \cdot & \cdot & \cdot & \beta_3 & k_2 \\ \cdot & \cdot & \cdot & \cdot & \gamma_2 & k_3 \\ \cdot & \cdot & \cdot & \cdot & \delta_1 & k_4 \\ -\alpha_4 & -\beta_3 & -\gamma_2 & -\delta_1 & 1 & k_5 \end{array} .$$

But by § 15 the determinant on the right

$$= \alpha_4^2 + \beta_3^2 + \gamma_2^2 + \delta_1^2 + \begin{vmatrix} \alpha_1 & \alpha_3 & \alpha_4 \\ & \beta_2 & \beta_3 \\ & & \delta_1 \end{vmatrix}^2 + \begin{vmatrix} \alpha_1 & \alpha_2 & \alpha_4 \\ & \beta_1 & \beta_3 \\ & & \gamma_2 \end{vmatrix}^2 + \begin{vmatrix} \alpha_2 & \alpha_3 & \alpha_4 \\ & \gamma_1 & \gamma_2 \\ & & \delta_1 \end{vmatrix}^2 + \begin{vmatrix} \beta_1 & \beta_2 & \beta_3 \\ & \gamma_1 & \gamma_2 \\ & & \delta_1 \end{vmatrix}^2,$$

and the bipartite

$$= h_5k_5 - k_5\frac{h_1h_2h_3h_4}{\alpha_4\beta_3\gamma_2\delta_1} + h_5\frac{k_1k_2k_3k_4}{\alpha_4\beta_3\gamma_2\delta_1};$$

and, further,

$$m(\alpha_4^2 + \beta_3^2 + \gamma_2^2 + \delta_1^2) - k_5\frac{h_1h_2h_3h_4}{\alpha_4\beta_3\gamma_2\delta_1} + h_5\frac{k_1k_2k_3k_4}{\alpha_4\beta_3\gamma_2\delta_1}$$

$$= \alpha_4\begin{vmatrix} m & h_1 & h_5 \\ & k_1 & k_5 \\ & & \alpha_4 \end{vmatrix} + \beta_3\begin{vmatrix} m & h_2 & h_5 \\ & k_2 & k_5 \\ & & \beta_3 \end{vmatrix} + \gamma_2\begin{vmatrix} m & h_3 & h_5 \\ & k_3 & k_5 \\ & & \gamma_2 \end{vmatrix} + \delta_1\begin{vmatrix} m & h_4 & h_5 \\ & k_4 & k_5 \\ & & \delta_1 \end{vmatrix}.$$

Consequently,

$$S_5 + B_5 = E_4 + h_5k_5 + \alpha_4\begin{vmatrix} m & h_1 & h_5 \\ & k_1 & k_5 \\ & & \alpha_4 \end{vmatrix} + \ldots$$

$$+ m\begin{vmatrix} \alpha_1 & \alpha_3 & \alpha_4 \\ & \beta_2 & \beta_3 \\ & & \delta_1 \end{vmatrix}^2 + \ldots$$

$$
= m + \frac{h_1h_2h_3h_4h_5}{k_1k_2k_3k_4k_5} + \Sigma\alpha_1 \left|\begin{array}{ccc} m & h_1 & h_2 \\ & k_1 & k_2 \\ & & \alpha_1 \end{array}\right| \quad (10 \text{ terms})
$$

$$
+ \, m\Sigma \left|\begin{array}{ccc} \alpha_1 & \alpha_2 & \alpha_3 \\ & \beta_1 & \beta_2 \\ & & \gamma_1 \end{array}\right|^2 \quad (5 \text{ terms})
$$

$$
= \mathrm{E}_5 .
$$

18. If E_5 be compared with the last of the expansions given in § 9, it will readily be seen that every term of the former is included in the latter. It follows from this that the terms of $\mathrm{S}_5 + \mathrm{B}_5$ are all terms of the skew determinant of the 6th order given in § 9. So far as S_5 is concerned, this is clear otherwise; the new point is that all the terms of

$$
\begin{array}{ccccc|c}
h_1 & h_2 & h_3 & h_4 & h_5 & \\
\hline
1 & \alpha_1 & \alpha_2 & \alpha_3 & \alpha_4 & k_1 \\
-\alpha_1 & 1 & \beta_1 & \beta_2 & \beta_3 & k_2 \\
-\alpha_2 & -\beta_1 & 1 & \gamma_1 & \gamma_2 & k_3 \\
-\alpha_3 & -\beta_2 & -\gamma_1 & 1 & \delta_1 & k_4 \\
-\alpha_4 & -\beta_3 & -\gamma_2 & -\delta_1 & 1 & k_5
\end{array}
\quad \text{are terms of} \quad
\left|\begin{array}{cccccc}
. & h_1 & h_2 & h_3 & h_4 & h_5 \\
-k_1 & 1 & \alpha_1 & \alpha_2 & \alpha_3 & \alpha_4 \\
-k_2 & -\alpha_1 & 1 & \beta_1 & \beta_2 & \beta_3 \\
-k_3 & -\alpha_2 & -\beta_1 & 1 & \gamma_1 & \gamma_2 \\
-k_4 & -\alpha_3 & -\beta_2 & -\gamma_1 & 1 & \delta_1 \\
-k_5 & -\alpha_4 & -\beta_3 & -\gamma_2 & -\delta_1 & 1
\end{array}\right| .
$$

19. On looking at the third of the identities in § 17, it is easily seen where certain of the terms on the right-hand side come from, viz.,

$$
m \quad \text{and} \quad m \left|\begin{array}{ccc} \alpha_1 & \alpha_2 & \alpha_3 \\ & \beta_1 & \beta_2 \\ & & \gamma_1 \end{array}\right|^2
$$

from the skew determinant, and $\frac{h_1h_2h_3h_4}{k_1k_2k_3k_4}$ from the bipartite. Leaving these out, we have remaining, as the essence of the identity,

$$
m(\alpha_1^2 + \alpha_2^2 + \alpha_3^2 + \beta_1^2 + \beta_2^2 + \gamma_1^2) +
\begin{array}{cccc|c}
h_1 & h_2 & h_3 & h_4 & \\
\hline
. & \alpha_1 & \alpha_2 & \alpha_3 & k_1 \\
-\alpha_1 & . & \beta_1 & \beta_2 & k_2 \\
-\alpha_2 & -\beta_1 & . & \gamma_1 & k_3 \\
-\alpha_3 & -\beta_2 & -\gamma_1 & . & k_4
\end{array}
= \Sigma\alpha_1 \left|\begin{array}{ccc} m & h_1 & h_2 \\ & k_1 & k_2 \\ & & \alpha_1 \end{array}\right| .
$$

On the Eliminant of $f(x)=0$, $f\left(\frac{1}{x}\right)=0$. By Thomas Muir, LL.D.

(Read December 7, 1896.)

1. In a paper "On the Existence of a Root of a Rational Integral Equation," published in the *Proc. Lond. Math. Soc.*, xxv. pp. 173–184, the author, Professor E. B. Elliott, says (p. 184) that it is unfortunate, for the simplicity of the argument of his paper, that a proof of a certain property of this eliminant, viz., that when two linear factors have been withdrawn from it, there is left a perfect square—"is one which direct algebraical methods have not yet supplied."

In the course of the following year the want referred to received attention, a demonstration being given by Mr W. W. Taylor, in a paper entitled "Evolution of a certain Dialytic Determinant," which was read before the same Society (see *Proc. Lond. Math. Soc.*, xxvii. pp. 60–66).

I purpose here giving another demonstration, which I think has the merit of bringing out more clearly the character of the constitution of the eliminant, and in which is followed, at the same time, that direct and expeditious course most likely to be taken by a student familiar with the theory of determinants.

2. Taking the case used in the second of the above-mentioned papers, viz., where

$$f(x)=ax^6+bx^5+cx^4+dx^3+ex^2+fx+g\,,$$

and where, therefore, the eliminant is

$$\begin{vmatrix} a & b & c & d & e & f & g & \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & a & b & c & d & e & f & g & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & a & b & c & d & e & f & g & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & a & b & c & d & e & f & g & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & a & b & c & d & e & f & g & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & a & b & c & d & e & f & g \\ g & f & e & d & c & b & a & \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & g & f & e & d & c & b & a & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & g & f & e & d & c & b & a & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & g & f & e & d & c & b & a & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & g & f & e & d & c & b & a & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & g & f & e & d & c & b & a \end{vmatrix}$$

we notice at once that, by reason of the centro-symmetry, the determinant resolves into

$$\begin{vmatrix} a & b & c & d & e & f+g \\ \cdot & a & b & c & d+g & e+f \\ \cdot & \cdot & a & b+g & c+f & d+e \\ \cdot & \cdot & g & a+f & b+e & c+d \\ \cdot & g & f & e & a+d & b+c \\ g & f & e & d & c & a+b \end{vmatrix}$$

and

$$\begin{vmatrix} a & b & c & d & e & f-g \\ \cdot & a & b & c & d-g & e-f \\ \cdot & \cdot & a & b-g & c-f & d-e \\ \cdot & \cdot & -g & a-f & b-e & c-d \\ \cdot & -g & -f & -e & a-d & b-c \\ -g & -f & -e & -d & -c & a-b \end{vmatrix} .$$

Of these it is clear that

$$a+b+c+d+e+f+g \quad \text{and} \quad a-b+c-d+e-f+g$$

are respectively factors, the cofactors being

$$\begin{vmatrix} a & b & c & d & e & 1 \\ \cdot & a & b & c & d+g & 1 \\ \cdot & \cdot & a & b+g & c+f & 1 \\ \cdot & \cdot & g & a+f & b+e & 1 \\ \cdot & g & f & e & a+d & 1 \\ g & f & e & d & c & 1 \end{vmatrix} , \qquad \begin{vmatrix} a & b & c & d & e & -1 \\ \cdot & a & b & c & d-g & 1 \\ \cdot & & a & b-g & c-f & -1 \\ \cdot & & -g & a-f & b-e & 1 \\ \cdot & -g & -f & -e & a-d & -1 \\ -g & -f & -e & -d & -c & 1 \end{vmatrix}$$

Diminishing (or increasing) each element of each row by the corresponding element of the next row, we change these into

$$\begin{vmatrix} a & b-a & c-b & d-c & e-d-g \\ \cdot & a & b-a & c-b-g & d+g-c-f \\ \cdot & \cdot & a-g & b+g-a-f & c+f-b-e \\ \cdot & -g & g-f & a+f-e & b+e-a-d \\ -g & g-f & f-e & e-d & a+d-c \end{vmatrix}$$

and

$$\begin{vmatrix} a & b+a & c+b & d+c & e+d-g \\ \cdot & a & b+a & c+b-g & d-g+c-f \\ \cdot & \cdot & a-g & b-g+a-f & c-f+b-e \\ \cdot & -g & -g-f & a-f-e & b-e+a-d \\ -g & -g-f & -f-e & -e-d & a-d-c \end{vmatrix},$$

and now increasing (or diminishing) each element of the 2nd column by the corresponding element of the 1st, each element of the 3rd by the corresponding element of the new 2nd, and so on, we have, finally, two identical determinants,

$$\begin{vmatrix} a & b & c & d & e-g \\ \cdot & a & b & c-g & d-f \\ \cdot & \cdot & a-g & b-f & c-e \\ \cdot & -g & -f & a-e & b-d \\ -g & -f & -e & -d & a-c \end{vmatrix} \text{ and } \begin{vmatrix} a & b & c & d & e-g \\ & a & b & c-g & d-f \\ & & a-g & b-f & c-e \\ & -g & -f & a-e & b-d \\ -g & -f & -e & -d & a-c \end{vmatrix};$$

so that the eliminant is equal to

$$(a+b+c+d+e+f+g)(a-b+c-d+e-f+g)$$
$$\times \begin{vmatrix} a & b & c & d & e-g \\ \cdot & a & b & c-g & d-f \\ \cdot & \cdot & a-g & b-f & c-e \\ \cdot & -g & -f & a-e & b-d \\ -g & -f & -e & -d & a-c \end{vmatrix}^2,$$

as was to be shown.

3. The essence of the proof is seen to be the resolution of the eliminant into two determinants closely resembling each other, the removal of a linear factor from each, and the reduction of the two co-factors into one and the same form.

The first of the two determinants is got from the eliminant by,

as it were, quadrisecting the latter by a vertical and a horizontal line, and folding the second or third quarter over on the first ;* so that, in fact, it may be written

$$\begin{vmatrix} a & b & c & d & e & f \\ & a & b & c & d & e \\ & & a & b & c & d \\ & & & a & b & c \\ & & & & a & b \\ & & & & & a \end{vmatrix} \;\#\; \begin{vmatrix} & & & & & g \\ & & & & g & f \\ & & & g & f & e \\ & & g & f & e & d \\ & g & f & e & d & c \\ g & f & e & d & c & b \end{vmatrix}$$

if the symbol $\#$ be taken to mean that each element of the one determinant-form is to be increased by the corresponding element of the other. The removal of the linear factor and the reduction of the determinant to the next lower order results in the curious theorem—

$$\begin{vmatrix} a & b & c & d & e & f \\ & a & b & c & d & e \\ & & a & b & c & d \\ & & & a & b & c \\ & & & & a & b \\ & & & & & a \end{vmatrix} \;\#\; \begin{vmatrix} & & & & & g \\ & & & & g & f \\ & & & g & f & e \\ & & g & f & e & d \\ & g & f & e & d & c \\ g & f & e & d & c & b \end{vmatrix}$$

$$=(a+b+c+d+e+f+g)$$

$$\times \begin{vmatrix} a & b & c & d & e \\ & a & b & c & d \\ & & a & b & c \\ & & & a & b \\ & & & & a \end{vmatrix} \;\#\; \begin{vmatrix} & & & & -g \\ & & & -g & -f \\ & & -g & -f & -e \\ & -g & -f & -e & -d \\ -g & -f & -e & -d & -c \end{vmatrix}.$$

The second of the two determinants differs from the first merely in having minus signs where the latter has plus signs, and the analogous theorem in regard to it is

* See the theorem regarding centro-symmetric determinants in Scott's Text-book, pp. 68, 69 ; or Muir's, pp. 183, 184. The original source is Zehfuss, *Zeitsch. f. Math. u. Phys.*, vii. pp. 438, 439, where, however, the case for odd-ordered determinants is quite erroneously stated.

$$\begin{vmatrix} a & b & c & d & e & f \\ & a & b & c & d & e \\ & & a & b & c & d \\ & & & a & b & c \\ & & & & a & b \\ & & & & & a \end{vmatrix} \# \begin{vmatrix} & & & & & -g \\ & & & & -g & -f \\ & & & -g & -f & -e \\ & & -g & -f & -e & -d \\ & -g & -f & -e & -d & -c \\ -g & -f & -e & -d & -c & -b \end{vmatrix}$$

$$= (a-b+c-d+e-f+g) \begin{vmatrix} a & b & c & d & e \\ & a & b & c & d \\ & & a & b & c \\ & & & a & b \\ & & & & a \end{vmatrix} \# \begin{vmatrix} & & & & -g \\ & & & -g & -f \\ & & -g & -f & -e \\ & -g & -f & -e & -d \\ -g & -f & -e & -d & -c \end{vmatrix}.$$

The square factor of the eliminant, left after the removal of $(a+b+c+d+\ldots)\ (a-b+c-d+\ldots)$, may thus be written—

$$\left\{ \begin{vmatrix} a & b & c & d & e \\ & a & b & c & d \\ & & a & b & c \\ & & & a & b \\ & & & & a \end{vmatrix} \# \begin{vmatrix} & & & & -g \\ & & & -g & -f \\ & & -g & -f & -e \\ & -g & -f & -e & -d \\ -g & -f & -e & -d & -c \end{vmatrix} \right\}^2 .$$

4. This squared factor possesses the property of remaining unaltered in substance on having its even-numbered elements, b, d, f, changed in sign. For, on making this alteration, the result is

$$\begin{vmatrix} a & -b & c & -d & e \\ & a & -b & c & -d \\ & & a & -b & c \\ & & & a & -b \\ & & & & a \end{vmatrix} \# \begin{vmatrix} & & & & -g \\ & & & -g & f \\ & & -g & f & -e \\ & -g & f & -e & d \\ -g & f & -e & d & -c \end{vmatrix},$$

and, as this cannot be affected in substance by changing the signs of the even-numbered rows and columns, we have it

$$= \begin{vmatrix} a & b & c & d & e \\ & a & b & c & d \\ & & a & b & c \\ & & & a & b \\ & & & & a \end{vmatrix} \# \begin{vmatrix} & & & & -g \\ & & & -g & -f \\ & & -g & -f & -e \\ & -g & -f & -e & -d \\ -g & -f & -e & -d & -c \end{vmatrix},$$

as stated.

From this and the relation between the linear factors, it immediately follows that the difference between the two original determinant factors lies in the fact that, if we view the first as a function of $a, b, c, d, e, f, g,$ the second is the very same function of $a, -b, c, -d, e, -f, g,$—a fact which is easily proved directly, as follows:—

The second determinant factor, as we have seen, is

$$\begin{vmatrix} a & b & c & d & e & f \\ & a & b & c & d & e \\ & & a & b & c & d \\ & & & a & b & c \\ & & & & a & b \\ & & & & & a \end{vmatrix} \# \begin{vmatrix} & & & & & -g \\ & & & & -g & -f \\ & & & -g & -f & -e \\ & & -g & -f & -e & -d \\ & -g & -f & -e & -d & -c \\ -g & -f & -e & -d & -c & -b \end{vmatrix}.$$

Now, this will not be altered in substance by changing the signs of all the elements in the odd-numbered rows and thereafter in the odd-numbered columns, so that it must be equal to

$$\begin{vmatrix} a & -b & c & -d & e & -f \\ & a & -b & c & -d & e \\ & & a & -b & c & -d \\ & & & a & -b & c \\ & & & & a & -b \\ & & & & & a \end{vmatrix} \# \begin{vmatrix} & & & & & g \\ & & & & g & -f \\ & & & g & -f & e \\ & & g & -f & e & -d \\ & g & -f & e & -d & c \\ g & -f & e & -d & c & -b \end{vmatrix},$$

and this is simply the first determinant factor with b, d, f replaced by $-b, -d, -f$.

5. The factorisation of the eliminant may thus be suitably epitomised as follows:—

$$\phi(a, b, c, d, e, f, g) = \psi(a, b, c, d, e, f, g) \, . \, \psi(a, -b, c, -d, e, -f, g),$$

$$= \left.\begin{matrix} (a+b+c+d+e+f+g)\cdot\chi(a, b, c, d, e, f, g) \\ (a-b+c-d+e-f+g)\cdot\chi(a, b, c, d, e, f, g) \end{matrix}\right\},$$

$$= (a+b+c+\ldots)(a-b+c-\ldots)\cdot\chi^2,$$

where ψ and χ are functions analogous to ϕ, viz.

$$\psi = \begin{vmatrix} a & b & c & d & e & f \\ & a & b & c & d & e \\ & & a & b & c & d \\ & & & a & b & c \\ & & & & a & b \\ & & & & & a \end{vmatrix} \# \begin{vmatrix} & & & & & g \\ & & & & g & f \\ & & & g & f & e \\ & & g & f & e & d \\ & g & f & e & d & c \\ g & f & e & d & c & b \end{vmatrix},$$

and

$$\chi = \begin{vmatrix} a & b & c & d & e \\ & a & b & c & d \\ & & a & b & c \\ & & & a & b \\ & & & & a \end{vmatrix} \# \begin{vmatrix} & & & & -g \\ & & & -g & -f \\ & & -g & -f & -e \\ & -g & -f & -e & -d \\ -g & -f & -e & -d & -c \end{vmatrix}.$$

6. Besides the results obtained in the preceding, viz.,

$$\begin{vmatrix} a & b \\ & a \end{vmatrix} \# \begin{vmatrix} & c \\ c & b \end{vmatrix} = (a+b+c)(a-c) = (a+b+c)\cdot\chi_1, \text{ say};$$

$$\begin{vmatrix} a & b & c \\ & a & b \\ & & a \end{vmatrix} \# \begin{vmatrix} & & d \\ & d & c \\ d & c & b \end{vmatrix} = (a+b+c+d)\cdot \begin{vmatrix} a & b \\ & a \end{vmatrix} \# \begin{vmatrix} & -d \\ -d & -c \end{vmatrix},$$

$$= (a+b+c+d)\cdot\chi_2, \text{ say};$$

$$\begin{vmatrix} a & b & c & d \\ & a & b & c \\ & & a & b \\ & & & a \end{vmatrix} \# \begin{vmatrix} & & & e \\ & & e & d \\ & e & d & c \\ e & d & c & b \end{vmatrix} = (a+b+c+d+e)\cdot \begin{vmatrix} a & b & c \\ & a & b \\ & & a \end{vmatrix} \# \begin{vmatrix} & & -e \\ & -e & -d \\ -e & -d & -c \end{vmatrix},$$

$$= (a+b+c+d+e)\cdot\chi_3, \text{ say};$$

. . . .

the following two additional sets are noteworthy, viz.,

$$\begin{vmatrix} a & b \\ & a \end{vmatrix} \# \begin{vmatrix} & b \\ b & a \end{vmatrix} = 2(a+b)(a-b),$$

$$\begin{vmatrix} a & b & c \\ & a & b \\ & & a \end{vmatrix} \# \begin{vmatrix} & & c \\ & c & b \\ c & b & a \end{vmatrix} = 2(a+b+c)(a-b+c)\cdot\chi_1,$$

$$\begin{vmatrix} a & b & c & d \\ & a & b & c \\ & & a & b \\ & & & a \end{vmatrix} \# \begin{vmatrix} & & & d \\ & & d & c \\ & d & c & b \\ d & c & b & a \end{vmatrix} = 2(a+b+c+d)(a-b+c-d)\cdot\chi_2,$$

. . . .

$$\begin{vmatrix} a & b \\ & a \end{vmatrix} \# \begin{vmatrix} & a \\ a & \end{vmatrix} = -ab,$$

$$\begin{vmatrix} a & b & c \\ & a & b \\ & & a \end{vmatrix} \# \begin{vmatrix} & & b \\ & b & a \\ b & a & \end{vmatrix} = -bc(a+b),$$

$$\begin{vmatrix} a & b & c & d \\ & a & b & c \\ & & a & b \\ & & & a \end{vmatrix} \# \begin{vmatrix} & & & c \\ & & c & b \\ & c & b & a \\ c & b & a & \end{vmatrix} = -cd(a+b+c)\cdot\chi_1,$$

$$\begin{vmatrix} a & b & c & d & e \\ & a & b & c & d \\ & & a & b & c \\ & & & a & b \\ & & & & a \end{vmatrix} \# \begin{vmatrix} & & & & d \\ & & & d & c \\ & & d & c & b \\ & d & c & b & a \\ d & c & b & a & \end{vmatrix} = -ed(a+b+c+d)\cdot\chi_2,$$

. . . .

The mode of proof necessary for these will be understood by considering a case of each.

(1) $$\begin{vmatrix} a & b & c & d & e \\ & a & b & c & d \\ & & a & b & c \\ & & & a & b \\ & & & & a \end{vmatrix} \# \begin{vmatrix} & & & & e \\ & & & e & d \\ & & e & d & c \\ & e & d & c & b \\ e & d & c & b & a \end{vmatrix} = 2(a+b+c+d+e)\begin{vmatrix} a & b & c & d & 1 \\ & a & b & c+e & 1 \\ & & a+e & b+d & 1 \\ & e & d & a+c & 1 \\ e & d & c & b & 1 \end{vmatrix},$$

$$= 2(a+b+c+d+e)\begin{vmatrix} a & b-a & c-b & d-c-e \\ \cdot & a & b-a-e & c+e-b-d \\ \cdot & -e & a+e-d & b+d-a-c \\ -e & e-d & d-c & a+c-b \end{vmatrix},$$

$$= 2(a+b+c+d+e)\begin{vmatrix} a & b & c & d-e \\ \cdot & a & b-e & c-d \\ \cdot & -e & a-d & b-c \\ -e & -d & -c & a-b \end{vmatrix},$$

$$= 2(a+b+c+d+e)\cdot\begin{vmatrix} a & b & c & d \\ & a & b & c \\ & & a & b \\ & & & a \end{vmatrix} \# \begin{vmatrix} & & & -e \\ & & -e & -d \\ & -e & -d & -c \\ -e & -d & -c & -b \end{vmatrix},$$

$$= 2(a+b+c+d+e)(a-b+c-d+e)\cdot\chi_3.$$

$$
(2)\quad \begin{vmatrix} a & b & c & d & e & f \\ & a & b & c & d & e \\ & & a & b & c & d \\ & & & a & b & c \\ & & & & a & b \\ & & & & & a \end{vmatrix} \mathbin{\#} \begin{vmatrix} & & & & & e \\ & & & & e & d \\ & & & e & d & c \\ & & e & d & c & b \\ & e & d & c & b & a \\ e & d & c & b & a & \end{vmatrix}
$$

$$
= \begin{vmatrix} a & b & c & d & e & f \\ & a & b & c & d+e & . \\ & & a & b+e & c+d & . \\ & & e & a+d & b+c & . \\ & e & d & c & a+b & . \\ e & d & c & b & a & . \end{vmatrix},
$$

$$
= -ef \begin{vmatrix} a & b & c & d+e \\ & a & b+e & c+d \\ & e & a+d & b+c \\ e & d & c & a+b \end{vmatrix},
$$

$$
= -ef \cdot \begin{vmatrix} a & b & c & d \\ & a & b & c \\ & & a & b \\ & & & a \end{vmatrix} \mathbin{\#} \begin{vmatrix} & & & e \\ & & e & d \\ & e & d & c \\ e & d & c & b \end{vmatrix},
$$

$$
= -ef(a+b+c+d+e) \cdot \chi_3 \, .
$$

It thus appears that for the evaluation of all the determinants of these three sets it is necessary only to know the final expansion of the set $\chi_1, \chi_2, \chi_3, \ldots$

On the Resolution of Circulants into Rational Factors.

By Thomas Muir, LL.D.

(Read December 7, 1896.)

(1) If we think of the circulant $C(a_1, a_2, \ldots, a_{n-1}, a_n)$ as the result of the elimination of x from the equations

$$\left.\begin{array}{r} a_1x^{n-1} + a_2x^{n-2} + \ldots\ldots + a_{n-1}x + a_n = 0 \\ x^n = 1 \end{array}\right\}$$

it is readily apparent that, corresponding to every rational factor of $x^n - 1$, there must be a rational factor of the circulant. Thus the circulant of the 6th order

$$\begin{vmatrix} a_1 & a_2 & a_3 & a_4 & a_5 & a_6 \\ a_6 & a_1 & a_2 & a_3 & a_4 & a_5 \\ a_5 & a_6 & a_1 & a_2 & a_3 & a_4 \\ a_4 & a_5 & a_6 & a_1 & a_2 & a_3 \\ a_3 & a_4 & a_5 & a_6 & a_1 & a_2 \\ a_2 & a_3 & a_4 & a_5 & a_6 & a_1 \end{vmatrix}$$

must have four rational factors, viz., the factor

$$a_1 + a_2 + a_3 + a_4 + a_5 + a_6$$

corresponding to the solution $x = 1$ of the equation $x^6 = 1$, the factor

$$a_1 - a_2 + a_3 - a_4 + a_5 - a_6$$

corresponding to the solution $x = -1$, and two other factors corresponding to the partial equations

$$x^2 + x + 1 = 0, \quad x^2 - x + 1 = 0.$$

The main object of this paper is the determination of such factors, and the presentation of them, when found, in the most suitable forms.

(2) It is clear, at the outset, that when n is odd we have always one linear factor, viz.,

$$a_1 + a_2 + \ldots\ldots + a_n,$$

and that when n is even we have two, viz.,

$$a_1 + a_2 + \ \ldots \ + a_n,$$
$$a_1 - a_2 + \ \ldots \ - a_n.$$

(3) The remaining factor, in the case where n is odd, is readily obtained by using along with the given linear equation the equation

$$\frac{x^n - 1}{x - 1} = 0,$$

instead of $x^n - 1 = 0$. Thus, when $n = 5$, we have

$$\text{and} \quad \left.\begin{array}{r} ax^4 + bx^3 + cx^2 + dx + e = 0 \\ x^4 + x^3 + x^2 + x + 1 = 0 \end{array}\right\}$$

whence it follows that

$$(b - a)x^3 + (c - a)x^2 + (d - a)x + (e - a) = 0,$$

and by cyclical substitution

$$(c - b)x^3 + (d - b)x^2 + (e - b)x + (a - b) = 0,$$
$$(d - c)x^3 + (e - c)x^2 + (a - c)x + (b - c) = 0,$$
$$(e - d)x^3 + (a - d)x^2 + (b - d)x + (c - d) = 0,$$

from which four equations we have the eliminant

$$\begin{vmatrix} b-a & c-a & d-a & e-a \\ c-b & d-b & e-b & a-b \\ d-c & e-c & a-c & b-c \\ e-d & a-d & b-d & c-d \end{vmatrix},$$

or (by diminishing each element of the first three columns by the corresponding element in the column immediately following),

$$\begin{vmatrix} b-c & c-d & d-e & e-a \\ c-d & d-e & e-a & a-b \\ d-e & e-a & a-b & b-c \\ e-a & a-b & b-c & c-d \end{vmatrix},$$

—a persymmetric determinant, ordinarily written

$$\mathrm{P}(b-c, c-d, d-e, e-a, a-b, b-c, c-d).*$$

* For further details regarding the co-factor of $a_1 + a_2 + \ldots + a_n$ in the case where n is odd, a paper on "Circulants of Odd Order" may be consulted in the *Quart. Journ. of Math.*, xviii. pp. 261–265.

(4) The remaining factor in the case where n is even is obtained in somewhat similar fashion. Thus, when $n=8$, we have

$$\left.\begin{array}{l} ax^7+bx^6+cx^5+dx^4+ex^3+fx^2+gx+h=0 \\ \quad\; x^6 \qquad\quad +x^4 \qquad\quad +x^2 \qquad\quad +1=0 \end{array}\right\},$$

and

so that, if we multiply both sides of the second equation by $ax+b$ and subtract, there results

$$(c-a)x^5+(d-b)x^4+(e-a)x^3+(f-b)x^2+(g-a)x+(h-b)=0,$$

whence, by cyclical substitution, we obtain sufficient equations to produce the eliminant

$$\begin{vmatrix} c-a & d-b & e-a & f-b & g-a & h-b \\ d-b & e-c & f-b & g-c & h-b & a-c \\ e-c & f-d & g-c & h-d & a-c & b-d \\ f-d & g-e & h-d & a-e & b-d & c-e \\ g-e & h-f & a-e & b-f & c-e & d-f \\ h-f & a-g & b-f & c-g & d-f & e-g \end{vmatrix},$$

or (by diminishing each element of the 1st column by the corresponding element of the 3rd column, each element of the 2nd column by the corresponding element of the 4th column, and so on),

$$\begin{vmatrix} c-e & d-f & e-g & f-h & g-a & h-b \\ d-f & e-g & f-h & g-a & h-b & a-c \\ e-g & f-h & g-a & h-b & a-c & b-d \\ f-h & g-a & h-b & a-c & b-d & c-e \\ g-a & h-b & a-c & b-d & c-e & d-f \\ h-b & a-c & b-d & c-e & d-f & e-g \end{vmatrix},$$

which is again a persymmetric determinant,

$$\mathrm{P}(c-e,\, d-f,\, e-g,\, f-h,\, g-a,\, h-b,\, a-c,\, b-d,\, c-e,\, d-f,\, e-g),$$

and is the factor desired.

(5) What has been done thus far suffices to give the rational factors in the cases $n=3, 4, 5$, the results being

$$\mathrm{C}(a_1, a_2, a_3)=(a_1+a_2+a_3)\begin{vmatrix} a_2-a_3 & a_3-a_1 \\ a_3-a_1 & a_1-a_2 \end{vmatrix},$$

$$C(a_1, a_2, a_3, a_4) = (a_1+a_2+a_3+a_4)(a_1-a_2+a_3-a_4)\begin{vmatrix} a_3-a_1 & a_4-a_2 \\ a_4-a_2 & a_1-a_3 \end{vmatrix},$$

$$C(a_1, \ldots\ldots, a_5) = (a_1+a_2+a_3+a_4+a_5)\begin{vmatrix} a_2-a_3 & a_3-a_4 & a_4-a_5 & a_5-a_1 \\ a_3-a_4 & a_4-a_5 & a_5-a_1 & a_1-a_2 \\ a_4-a_5 & a_5-a_1 & a_1-a_2 & a_2-a_3 \\ a_5-a_1 & a_1-a_2 & a_2-a_3 & a_3-a_4 \end{vmatrix}.$$

(6) In the case where $n=6$ two factors are as yet unknown, viz., the two corresponding to the factors x^2+x+1, x^2-x+1 of x^6-1. For finding the first of the two, the equations concerned are

$$\left.\begin{aligned} a_1x^5+a_2x^4+a_3x^3+a_4x^2+a_5x+a_6=0 \\ x^2+\quad x+\ 1=0 \end{aligned}\right\}.$$

With the help of the second equation the term a_1x^5 can be removed from the first, the result being

$$(a_2-a_1)x^4+(a_3-a_1)x^3+a_4x^2+a_5x+a_6=0.$$

From this equation, again, the term $(a_2-a_1)x^4$ can be removed in the same way with the result

$$(a_3-a_2)x^3+(a_4-a_2+a_1)x^2+a_5x+a_6=0,$$

and this process can clearly be continued until there occurs in the resulting equation no power of x higher than the first. Doing so, we have finally

$$(a_5-a_4+a_2-a_1)x+(a_6-a_4+a_3-a_1)=0,$$

and, by cyclical substitution,

$$(a_6-a_5+a_3-a_2)x+(a_1-a_5+a_4-a_2)=0,$$

from which two equations there results the eliminant

$$\begin{vmatrix} a_5-a_4+a_2-a_1 & a_6-a_4+a_3-a_1 \\ a_6-a_5+a_3-a_2 & a_1-a_5+a_4-a_2 \end{vmatrix},$$

or, in persymmetric form,

$$\begin{vmatrix} a_3-a_4+a_6-a_1 & a_4-a_5+a_1-a_2 \\ a_4-a_5+a_1-a_2 & a_5-a_6+a_2-a_3 \end{vmatrix},$$

and this is the desired factor.

By combining the various subtractions in this process, the work

required may be considerably condensed. In fact, what has been done is really equivalent to multiplying

$$x^2+x+1$$

by

$$a_1x^3+(a_2-a_1)x^2+(a_3-a_2)x+(a_4-a_3+a_1)\,,$$

and subtracting the product

$$a_1x^5+a_2x^4+a_3x^3+a_4x^2+(a_4-a_2+a_1)x+(a_4-a_3+a_1)$$

from

$$a_1x^5+a_2x^4+a_3x^3+a_4x^2+\qquad a_5x+\qquad a_6\,.$$

Moreover, when put in this way, the process closely resembles that followed in the previous cases.

The factor corresponding to x^2-x+1 is similarly found to be

$$\begin{vmatrix} a_5+a_4-a_2-a_1 & a_6-a_4-a_5+a_1 \\ a_6+a_5-a_3-a_2 & a_1-a_5-a_4+a_2 \end{vmatrix},$$

or, in persymmetric form,

$$\begin{vmatrix} -a_3-a_4+a_6+a_1 & -a_4-a_5+a_1+a_2 \\ -a_4-a_5+a_1+a_2 & -a_5-a_6+a_2+a_3 \end{vmatrix},$$

so that as a final result we have

$$\begin{aligned} C(a_1\,,a_2\,,a_3\,,a_4\,,a_5\,,a_6) = {} & (a_1+a_2+a_3+a_4+a_5+a_6) \\ & \cdot\,(a_1-a_2+a_3-a_4-a_5+a_6) \\ & \cdot \begin{vmatrix} a_3-a_4+a_6-a_1 & a_4-a_5+a_1-a_2 \\ a_4-a_5+a_1-a_2 & a_3-a_6+a_2-a_3 \end{vmatrix} \\ & \cdot \begin{vmatrix} -a_3-a_4+a_6+a_1 & -a_4-a_5+a_1+a_2 \\ -a_4-a_5+a_1+a_2 & -a_5-a_6+a_2+a_3 \end{vmatrix}. \end{aligned}$$

(7) The case where $n=7$ presents no new feature, 7 being like 5 a prime. The result thus is

$$C(a_1a_2\,,\ldots,a_7)=(a_1+a_2+\ldots+a_7)\cdot P(a_2-a_3\,,a_3-a_4\,,\ldots,a_7-a_1\,,a_1-a_2\,,\ldots,a_5-a_6).$$

(8) The lengthened process given in § 6 suffices to show not only that for every rational factor of x^n-1 there must be a rational factor of $C(a_1\,,a_2\,,\ldots,a_n)$, but also that the degree of corresponding factors must be the same.

Thus, to take the next case, the mere fact that x^8-1 is resolvable into

$$(x-1)(x+1)(x^2+1)(x^4+1)$$

necessitates that $C(a_1, a_2, \ldots, a_8)$ should be resolvable also into four factors, and further, that there also must be two of the 1st degree, one of the 2nd, and one of the 4th. For the sake of clearness it may be well to go through the reasoning again, say for the case of the factor x^4+1 :—

The equations concerned are

$$\left.\begin{array}{r} a_1x^7 + a_2x^6 + a_3x^5 + \ldots + a_8 = 0 \\ x^4 + 1 = 0 \end{array}\right\},$$

and, since in the first equation -1 may be substituted for x^4, it follows that the highest power of x left after the substitutions have been made must be the 3rd. That is to say, the final equation must be of the form

$$A_1x^3 + A_2x^2 + A_3x + A_4 = 0 .$$

The cyclical substitution will then give three other similar equations, and the eliminant obtained will be a determinant of the 4th order, having each of its elements of the first degree.

The actual complete result is

$$\begin{aligned} C(a_1, a_2, a_3, \ldots, a_8) = & (a_1+a_2+a_3+ \ldots +a_8) \\ & \cdot (a_1-a_2+a_3- \ldots -a_8) \\ & \cdot \begin{vmatrix} a_7-a_5+a_3-a_1 & a_8-a_6+a_4-a_2 \\ a_8-a_6+a_4-a_2 & a_1-a_7+a_5-a_3 \end{vmatrix} \\ & \begin{vmatrix} a_5-a_1 & a_6-a_2 & a_7-a_3 & a_8-a_4 \\ a_6-a_2 & a_7-a_3 & a_8-a_4 & a_1-a_5 \\ a_7-a_3 & a_8-a_4 & a_1-a_5 & a_2-a_6 \\ a_8-a_4 & a_1-a_5 & a_2-a_6 & a_3-a_7 \end{vmatrix}, \end{aligned}$$

where it has again to be observed that the determinants are persymmetric.

The results for the cases from $n=2$ to $n=10$ may be tabulated as follows :—

	Factors of x^n-1	Factors of $C(a_1, \ldots, a_n)$
$n=2$	$x-1$ $x+1$	a_1+a_2 a_1-a_2

$n=3$	$x-1$ x^2+x+1	$a_1+a_2+a_3$ $P(a_2-a_3,\ a_3-a_1,\ a_1-a_2)$
$n=4$	$x-1$ $x+1$ x^2+1	$a_1+a_2+a_3+a_4$ $a_1-a_2+a^3-a_4$ $P(a_3-a_1,\ a_4-a_2,\ a_1-a_3)$
$n=5$	$x-1$ $x^4+x^3+\ldots+1$	$a_1+a_2+\ldots+a_5$ $P(a_2-a_3,\ a_3-a_4,\ \ldots,\ a_5-a_1,\ a_1-a_2,\ \ldots,\ a_3-a_4)$
$n=6$	$x-1$ $x+1$ x^2+x+1 x^2-x+1	$a_1+a_2+\ldots+a_6$ $a_1-a_2+\ldots-a_6$ $P(a_3-a_4+a_6-a_1,\ a_4-a_5+a_1-a_2,\ a_5-a_6+a_2-a_3)$ $P(-a_3-a_4+a_6+a_1,\ -a_4-a_5+a_1+a_2,\ -a_5-a_6+a_2+a_3)$
$n=7$	$x-1$ $x^6+x^5+\ldots+1$	$a_1+a_2+\ldots+a_7$ $P(a_2-a_3;\ a_3-a_4,\ \ldots,\ a_6-a_7,\ a_7-a_1,\ a_1-a_2,\ \ldots,\ a_5-a_6)$
$n=8$	$x-1$ $x+1$ x^2+1 x^4+1	$a_1+a_2+\ldots+a_8$ $a_1-a_2+\ldots-a_5$ $P(a_7-a_5+a_3-a_1,\ a_8-a_6+a_4-a_2,\ a_1-a_7+a_5-a_3)$ $P(a_5-a_1,\ a_6-a_2,\ \ldots,\ a_8-a_4,\ a_1-a_5,\ \ldots,\ a_3-a_7)$
$n=9$	$x-1$ x^2+x+1 x^6+x^3+1	$a_1+a_2+\ldots+a_9$ $P(a_9-a_2+a_6-a_4+a_3-a_1,\ a_1-a_8+a_7-a_5+a_4-a_2,\ a_2-a_9+a_8-a_6+a_5-a_3)$ $P(a_4-a_7,\ a_5-a_8,\ \ldots,\ a_9-a_3,\ a_1-a_4,\ \ldots,\ a_5-a_8)$
$n=10$	$x-1$ $x+1$ $x^4+x^3+\ldots+1$ $x^4-x^3+\ldots+1$	$a_1+a_2+\ldots+a_{10}$ $a_1-a_2+\ldots-a_{10}$ $P(a_7-a_8+a_2-a_3,\ a_8-a_9+a_3-a_4,\ \ldots,\ a_3-a_4+a_8-a_9)$ $P(a_7+a_8-a_2-a_3,\ a_8+a_9-a_3-a_4,\ \ldots,\ a_3+a_4-a_8-a_9)$

It is well worthy of notice that all the persymmetric determinants here given would be sufficiently specified by writing *only the first element*, the others being obtained at once therefrom by the cyclical substitution. For example, when $n=10$, the factor of $C(a_1, \cdots, a_{10})$ corresponding to the factors $x^4+x^3+x^2+x+1$ of $x^{10}-1$ is known to be a persymmetric determinant of the 4th order; and, consequently, if it be further known that the first element is

$a_7-a_8+a_2-a_3$, the whole determinant factor can be at once written, viz.,

$$\begin{vmatrix} a_7-a_8+a_2-a_3 & a_8-a_9+a_3-a_4 & a_9-a_{10}+a_4-a_5 & a_{10}-a_1+a_5-a_6 \\ a_8-a_9+a_3-a_4 & a_9-a_{10}+a_4-a_5 & a_{10}-a_1+a_5-a_6 & a_1-a_2+a_6-a_7 \\ a_9-a_{10}+a_4-a_5 & a_{10}-a_1+a_5-a_6 & a_1-a_2+a_6-a_7 & a_2-a_3+a_7-a_8 \\ a_{10}-a_1+a_5-a_6 & a_1-a_2+a_6-a_7 & a_2-a_3+a_7-a_8 & a_3-a_4+a_8-a_9 \end{vmatrix}.$$

(9) All these identities, setting forth the resolution of circulants into rational factors, should, of course, be demonstrable by application of the laws of determinants alone without any reference to the principles of elimination. The *discovering* of the factors in this way would not, as a rule, be an easy matter, but the establishment of the correctness of the resolution, when once accomplished in the manner of the preceding paragraphs, is not exceptionally troublesome.

As an example the case where $n=8$ may be taken, the problem being to resolve

$$\begin{vmatrix} a_1 & a_2 & a_3 & a_4 & a_5 & a_6 & a_7 & a_8 \\ a_8 & a_1 & a_2 & a_3 & a_4 & a_5 & a_6 & a_7 \\ a_7 & a_8 & a_1 & a_2 & a_3 & a_4 & a_5 & a_6 \\ a_6 & a_7 & a_8 & a_1 & a_2 & a_3 & a_4 & a_5 \\ a_5 & a_6 & a_7 & a_8 & a_1 & a_2 & a_3 & a_4 \\ a_4 & a_5 & a_6 & a_7 & a_8 & a_1 & a_2 & a_3 \\ a_3 & a_4 & a_5 & a_6 & a_7 & a_8 & a_1 & a_2 \\ a_2 & a_3 & a_4 & a_5 & a_6 & a_7 & a_8 & a_1 \end{vmatrix}$$

into the three factors of § 4 and thereafter into the four of § 8.

(10) The process of getting the first factor is well known. We have only to add to each element of the first column the corresponding element of all the other columns when the desired factor is at once got, and we have left the co-factor

$$\begin{vmatrix} 1 & a_2 & a_3 & a_4 & a_5 & a_6 & a_7 & a_8 \\ 1 & a_1 & a_2 & a_3 & a_4 & a_5 & a_6 & a_7 \\ 1 & a_8 & a_1 & a_2 & a_3 & a_4 & a_5 & a_6 \\ 1 & a_7 & a_8 & a_1 & a_2 & a_3 & a_4 & a_5 \\ 1 & a_6 & a_7 & a_8 & a_1 & a_2 & a_3 & a_4 \\ 1 & a_5 & a_6 & a_7 & a_8 & a_1 & a_2 & a_3 \\ 1 & a_4 & a_5 & a_6 & a_7 & a_8 & a_1 & a_2 \\ 1 & a_3 & a_4 & a_5 & a_6 & a_7 & a_8 & a_1 \end{vmatrix}.$$

Similarly, by adding to each element of the 1st row the corresponding element of all the odd-numbered rows, and thereafter subtracting the corresponding element of all the even-numbered rows, the second factor can be removed, with the result that the co-factor left is

$$\begin{vmatrix} 0 & -1 & 1 & -1 & 1 & -1 & 1 & -1 \\ 1 & a_1 & a_2 & a_3 & a_4 & a_5 & a_6 & a_7 \\ 1 & a_8 & a_1 & a_2 & a_3 & a_4 & a_5 & a_6 \\ 1 & a_7 & a_8 & a_1 & a_2 & a_3 & a_4 & a_4 \\ 1 & a_6 & a_7 & a_8 & a_1 & a_2 & a_3 & a_4 \\ 1 & a_5 & a_6 & a_7 & a_8 & a_1 & a_2 & a_3 \\ 1 & a_4 & a_5 & a_6 & a_7 & a_8 & a_1 & a_2 \\ 1 & a_3 & a_4 & a_5 & a_6 & a_7 & a_8 & a_1 \end{vmatrix} .$$

Now, subtracting from each element of the 2nd row in this the corresponding element of the 3rd, and from each element of the 3rd the corresponding element of the 4th, and so on, we obtain

$$-\begin{vmatrix} -1 & 1 & -1 & \cdot\ \cdot\ \cdot\ \cdot & -1 \\ a_1 - a_8 & a_2 - a_1 & a_3 - a_2 & \cdot\ \cdot\ \cdot\ \cdot & a_7 - a_6 \\ a_8 - a_7 & a_1 - a_8 & a_2 - a_1 & \cdot\ \cdot\ \cdot\ \cdot & a_6 - a_5 \\ a_7 - a_6 & a_8 - a_7 & a_1 - a_8 & \cdot\ \cdot\ \cdot\ \cdot & a_5 - a_4 \\ a_6 - a_5 & a_7 - a_6 & a_8 - a_7 & \cdot\ \cdot\ \cdot\ \cdot & a_4 - a_3 \\ a_5 - a_4 & a_6 - a_5 & a_7 - a_6 & \cdot\ \cdot\ \cdot\ \cdot & a_3 - a_2 \\ a_4 - a_3 & a_5 - a_4 & a_6 - a_5 & \cdot\ \cdot\ \cdot\ \cdot & a_2 - a_1 \end{vmatrix} ,$$

and, similarly, by adding to each element of the 1st column the corresponding element of the 2nd column, to each element of the 2nd column the corresponding element of the 3rd column, and so on, there results

$$\begin{vmatrix} a_2 - a_8 & a_3 - a_1 & a_4 - a_2 & a_5 - a_3 & a_6 - a_4 & a_7 - a_5 \\ a_1 - a_7 & a_2 - a_8 & a_3 - a_1 & a_4 - a_2 & a_5 - a_3 & a_6 - a_4 \\ a_8 - a_6 & a_1 - a_7 & a_2 - a_8 & a_3 - a_1 & a_4 - a_2 & a_5 - a_3 \\ a_7 - a_5 & a_8 - a_6 & a_1 - a_7 & a_2 - a_8 & a_3 - a_1 & a_4 - a_2 \\ a_6 - a_4 & a_7 - a_5 & a_8 - a_6 & a_1 - a_7 & a_2 - a_8 & a_3 - a_1 \\ a_5 - a_3 & a_6 - a_4 & a_7 - a_5 & a_8 - a_6 & a_1 - a_7 & a_2 - a_8 \end{vmatrix} ,$$

or, on reversing the order of the rows and changing the signs of all the elements,

$$- \begin{vmatrix} a_3 - a_5 & a_4 - a_6 & a_5 - a_7 & a_6 - a_8 & a_7 - a_1 & a_8 - a_2 \\ a_4 - a_6 & a_5 - a_7 & a_6 - a_8 & a_7 - a_1 & a_8 - a_2 & a_1 - a_3 \\ a_5 - a_7 & a_6 - a_8 & a_7 - a_1 & a_8 - a_2 & a_1 - a_3 & a_2 - a_4 \\ a_6 - a_8 & a_7 - a_1 & a_8 - a_2 & a_1 - a_3 & a_2 - a_4 & a_3 - a_5 \\ a_7 - a_1 & a_8 - a_2 & a_1 - a_3 & a_2 - a_4 & a_3 - a_5 & a_4 - a_6 \\ a_8 - a_2 & a_1 - a_3 & a_2 - a_4 & a_3 - a_5 & a_4 - a_6 & a_5 - a_7 \end{vmatrix},$$

which is exactly the same as the determinant of § 4.

Finally, by increasing each element of the 1st column by the corresponding element of the 5th, and each element of the 2nd column by the corresponding element of the 6th; and thereafter diminishing each element of the 6th row by the corresponding element of the 4th, each element of the 5th row by the corresponding element of the 3rd, and so on, we have

$$\begin{vmatrix} a_7 - a_1 + a_3 - a_5 & a_8 - a_2 + a_4 - a_6 & a_5 - a_7 & a_6 - a_8 & a_7 - a_1 & a_8 - a_2 \\ a_8 - a_2 + a_4 - a_6 & a_1 - a_3 + a_5 - a_7 & a_6 - a_8 & a_7 - a_1 & a_8 - a_2 & a_1 - a_3 \\ \cdot & \cdot & a_5 - a_1 & a_6 - a_2 & a_7 - a_3 & a_8 - a_4 \\ \cdot & \cdot & a_6 - a_2 & a_7 - a_3 & a_8 - a_4 & a_1 - a_5 \\ \cdot & \cdot & a_7 - a_3 & a_8 - a_4 & a_1 - a_5 & a_2 - a_6 \\ \cdot & \cdot & a_8 - a_4 & a_1 - a_5 & a_2 - a_6 & a_3 - a_7 \end{vmatrix},$$

which clearly resolves into the two determinants of § 8, viz.,

$$P(a_7 - a_1 + a_3 - a_5,\ a_8 - a_2 + a_4 - a_6,\ a_1 - a_3 + a_5 - a_7),$$
$$P(a_5 - a_1,\ a_6 - a_2, \ldots .\ a_8 - a_4,\ a_1 - a_5, \ldots ,\ a_3 - a_7).$$

(11) We come now to a very interesting property of circulants of the form

$$C(a_1,\ a_2,\ a_3,\ \ldots,\ a_3,\ a_2),$$

that is to say, circulants whose first row, after the element in the place (1, 1) has been deleted, is the same when read backwards as when read forwards. This property is to the effect that the co-factor of

$$a_1 + a_2 + a_3 + \ldots + a_3 + a_2$$

in the case of a circulant of odd order, and the co-factor of

$$(a_1 + a_2 + a_3 + \ldots + a_3 + a_2)\ (a_1 - a_2 + a_3 - \ldots + a_3 - a_2)$$

in the case of a circulant of even order are complete squares.

As an example of the former case let us take the circulant of the 7th order, viz.,

$$C(a_1,\ a_2,\ a_3,\ a_4,\ a_5,\ a_6,\ a_7)$$

or, for the sake of symmetry afterwards,

$$C(a_5, a_6, a_7, a_1, a_2, a_3, a_4).$$

The co-factor of $a_5 + a_6 + a_7 + a_1 + a_2 + a_3 + a_4$ we know to be

$$\begin{vmatrix} a_5 - a_4 & a_6 - a_5 & a_7 - a_6 & a_1 - a_7 & a_2 - a_1 & a_3 - a_2 \\ a_4 - a_3 & a_5 - a_4 & a_6 - a_5 & a_7 - a_6 & a_1 - a_7 & a_2 - a_1 \\ a_3 - a_2 & a_4 - a_3 & a_5 - a_4 & a_6 - a_5 & a_7 - a_6 & a_1 - a_7 \\ a_2 - a_1 & a_3 - a_2 & a_4 - a_3 & a_5 - a_4 & a_6 - a_5 & a_7 - a_6 \\ a_1 - a_7 & a_2 - a_1 & a_3 - a_2 & a_4 - a_3 & a_5 - a_4 & a_6 - a_5 \\ a_7 - a_6 & a_1 - a_7 & a_2 - a_1 & a_3 - a_2 & a_4 - a_3 & a_5 - a_4 \end{vmatrix}$$

which, on putting $a_7, a_6, a_5 = a_2, a_3, a_4$ becomes

$$\begin{vmatrix} \cdot & a_3 - a_4 & a_2 - a_3 & a_1 - a_2 & a_2 - a_1 & a_3 - a_2 \\ a_4 - a_3 & \cdot & a_3 - a_4 & a_2 - a_3 & a_1 - a_2 & a_2 - a_1 \\ a_3 - a_2 & a_4 - a_3 & \cdot & a_3 - a_4 & a_2 - a_3 & a_1 - a_2 \\ a_2 - a_1 & a_3 - a_2 & a_4 - a_3 & \cdot & a_3 - a_4 & a_2 - a_3 \\ a_1 - a_2 & a_2 - a_1 & a_3 - a_2 & a_4 - a_3 & \cdot & a_3 - a_4 \\ a_2 - a_3 & a_1 - a_2 & a^2 - a_1 & a_3 - a_2 & a_4 - a_3 & \cdot \end{vmatrix},$$

and this, by Cayley's theorem regarding zero-axial skew determinants of even order, is equal to

$$\left|\begin{array}{ccccc} a_3 - a_4 & a_2 - a_3 & a_1 - a_2 & a_2 - a_1 & a_3 - a_2 \\ & a_3 - a_4 & a_2 - a_3 & a_1 - a_2 & a_2 - a_1 \\ & & a_3 - a_4 & a_2 - a_3 & a_1 - a_2 \\ & & & a_3 - a_4 & a_2 - a_3 \\ & & & & a_3 - a_4 \end{array}\right|^2.$$

(12) As an example of an even-ordered circulant, let us take the case of $n = 8$. The two linear factors having been removed, the remaining factor we know from § 9 to be

$$\begin{vmatrix} a_2 - a_8 & a_3 - a_1 & a_4 - a_2 & a_5 - a_3 & a_6 - a_4 & a_7 - a_5 \\ a_1 - a_7 & a_2 - a_8 & a_3 - a_1 & a_4 - a_2 & a_5 - a_3 & a_6 - a_4 \\ a_8 - a_6 & a_1 - a_7 & a_2 - a_8 & a_3 - a_1 & a_4 - a_2 & a_5 - a_3 \\ a_7 - a_5 & a_8 - a_6 & a_1 - a_7 & a_2 - a_8 & a_3 - a_1 & a_4 - a_2 \\ a_6 - a_4 & a_7 - a_5 & a_8 - a_6 & a_1 - a_7 & a_2 - a_8 & a_3 - a_1 \\ a_5 - a_3 & a_6 - a_4 & a_7 - a_5 & a_8 - a_6 & a_1 - a_7 & a_2 - a_8 \end{vmatrix},$$

and this, on putting $a_8, a_7, a_6 = a_2, a_3, a_4$ becomes

$$\begin{vmatrix} \cdot & a_3-a_1 & a_4-a_2 & a_5-a_3 & \cdot & a_3-a_5 \\ a_1-a_3 & \cdot & a_3-a_1 & a_4-a_2 & a_5-a_3 & \cdot \\ a_2-a_4 & a_1-a_3 & \cdot & a_3-a_1 & a_4-a_2 & a_5-a_3 \\ a_3-a_5 & a_2-a_4 & a_1-a_3 & \cdot & a_3-a_1 & a_4-a_2 \\ \cdot & a_3-a_5 & a_2-a_4 & a_1-a_3 & \cdot & a_3-a_1 \\ a_5-a_3 & \cdot & a_3-a_5 & a_2-a_4 & a_1-a_3 & \cdot \end{vmatrix},$$

which, again, by Cayley's theorem is equal to

$$\begin{vmatrix} a_3-a_1 & a_4-a_2 & a_5-a_3 & \cdot & a_3-a_5 \\ & a_3-a_1 & a_4-a_2 & a_5-a_3 & \cdot \\ & & a_3-a_1 & a_4-a_2 & a_5-a_3 \\ & & & a_3-a_1 & a_4-a_2 \\ & & & & a_3-a_1 \end{vmatrix}^2.$$

(13) Another mode of attaining these results is worthy of attention, not merely because of the interest attaching to a different method, but because it gives the square in a totally different form.

Taking, as before, the circulant of the 7th order, and observing that it is centro-symmetric, we resolve it at once into two determinants, viz.,

$$\begin{vmatrix} a_1+a_2 & a_2+a_3 & a_3+a_4 & a_4 \\ a_2+a_3 & a_1+a_4 & a_2+a_4 & a_3 \\ a_3+a_4 & a_2+a_4 & a_1+a_3 & a_2 \\ 2a_4 & 2a_3 & 2a_2 & a_1 \end{vmatrix}, \quad \begin{vmatrix} a_1-a_2 & a_2-a_3 & a_3-a_4 \\ a_2-a_3 & a_1-a_4 & a_2-a_4 \\ a_3-a_4 & a_2-a_4 & a_1-a_3 \end{vmatrix}.$$

The former of these, however, is equal to

$$(a_1+2a_2+2a_3+2a_4)\begin{vmatrix} a_1+a_2 & a_2+a_3 & a_3+a_4 & 1 \\ a_2+a_3 & a_1+a_4 & a_2+a_4 & 1 \\ a_3+a_4 & a_2+a_4 & a_1+a_3 & 1 \\ 2a_4 & 2a_3 & 2a_2 & 1 \end{vmatrix},$$

and, by subtracting each element of the 2nd, 3rd, and 4th rows from the corresponding element in the row preceding it, this is readily transformed into

$$(a_1+2a_2+2a_3+2a_4)\begin{vmatrix} a_1-a_3 & a_2+a_3-a_1-a_4 & a_3-a_2 \\ a_2-a_4 & a_1-a_2 & a_2+a_4-a_1-a_3 \\ a_3-a_4 & a_2+a_4-2a_3 & a_1+a_3-2a_2 \end{vmatrix}.$$

Increasing each element of the last column by the corresponding

elements of the other columns, and each element of the 2nd column by the corresponding element of the 1st column, we have

$$(a_1+2a_2+2a_3+2a_4)\begin{vmatrix} a_1-a_3 & a_2-a_4 & a_3-a_4 \\ a_2-a_4 & a_1-a_4 & a_2-a_3 \\ a_3-a_4 & a_2-a_3 & a_1-a_2 \end{vmatrix},$$

the determinant in which is exactly the same as the second of the two factors we started with. The theorem is thus established, and a comparison with the result of § 10 leads us to the curious identity

$$\begin{vmatrix} a_1-a_3 & a_2-a_4 & a_3-a_4 \\ a_2-a_4 & a_1-a_4 & a_2-a_3 \\ a_3-a_4 & a_2-a_3 & a_1-a_2 \end{vmatrix} = \begin{vmatrix} a_3-a_4 & a_2-a_3 & a_1-a_2 & a_2-a_1 & a_3-a_2 \\ & a_3-a_4 & a_2-a_3 & a_1-a_2 & a_2-a_1 \\ & & a_3-a_4 & a_2-a_3 & a_1-a_2 \\ & & & a_3-a_4 & a_2-a_3 \\ & & & & a_3-a_4 \end{vmatrix}.$$

(14) Taking now an even-ordered circulant,—say, as before, the circulant of the 8th order,

$$C(a_1,\ a_2,\ a_3,\ a_4,\ a_5,\ a_4,\ a_3,\ a_2)$$

we see again, by reason of the centro-symmetry, that it is resolvable into

$$\begin{vmatrix} a_1+a_2 & a_2+a_3 & a_3+a_4 & a_4+a_5 \\ a_2+a_3 & a_1+a_4 & a_2+a_5 & a_3+a_4 \\ a_3+a_4 & a_2+a_5 & a_1+a_4 & a_2+a_3 \\ a_4+a_5 & a_3+a_4 & a_2+a_3 & a_1+a_2 \end{vmatrix} \cdot \begin{vmatrix} a_1-a_2 & a_2-a_3 & a_3-a_4 & a_4-a_5 \\ a_2-a_3 & a_1-a_4 & a_2-a_5 & a_3-a_4 \\ a_3-a_4 & a_2-a_5 & a_1-a_4 & a_2-a_3 \\ a_4-a_5 & a_3-a_4 & a_2-a_3 & a_1-a_2 \end{vmatrix}.$$

From each of these a known linear factor is separable, the result being

$$(a_1+2a_2+2a_3+2a_4+a_5)\,(a_1-2a_2+2a_3-2a_4+a_5)$$

$$\times\begin{vmatrix} a_1+a_2 & a_2+a_3 & a_3+a_4 & 1 \\ a_2+a_3 & a_1+a_4 & a_2+a_5 & 1 \\ a_3+a_4 & a_2+a_5 & a_1+a_4 & 1 \\ a_4+a_5 & a_3+a_4 & a_2+a_3 & 1 \end{vmatrix} \cdot \begin{vmatrix} a_1-a_2 & a_2-a_3 & a_3-a_4 & -1 \\ a_2-a_3 & a_1-a_4 & a_2-a_5 & 1 \\ a_3-a_4 & a_2-a_5 & a_1-a_4 & -1 \\ a_4-a_5 & a_3-a_4 & a_2-a_3 & 1 \end{vmatrix}.$$

These two determinants, however, are clearly transformable into

$$\begin{vmatrix} a_1-a_3 & a_2+a_3-a_1-a_4 & a_3+a_4-a_2-a_5 \\ a_2-a_4 & a_1+a_4-a_2-a_5 & a_2+a_5-a_1-a_4 \\ a_3-a_5 & a_2+a_5-a_3-a_4 & a_1+a_4-a_2-a_3 \end{vmatrix}, \quad \begin{vmatrix} a_1-a_3 & a_2-a_3+a_1-a_4 & a_3-a_4+a_2-a_5 \\ a_2-a_4 & a_1-a_4+a_2-a_5 & a_2-a_5+a_1-a_4 \\ a_3-a_5 & a_2-a_5+a_3-a_4 & a_1-a_4+a_2-a_3 \end{vmatrix}$$

and therefore into

$$\begin{vmatrix} a_1-a_3 & a_2-a_4 & a_3-a_5 \\ a_2-a_4 & a_1-a_5 & a_2-a_4 \\ a_3-a_5 & a_2-a_4 & a_1-a_3 \end{vmatrix}, \qquad \begin{vmatrix} a_1-a_3 & a_2-a_4 & a_3-a_5 \\ a_2-a_4 & a_1-a_5 & a_2-a_4 \\ a_3-a_5 & a_2-a_4 & a_1-a_3 \end{vmatrix},$$

and are thus identical, as was to be proved.

By comparison with the result of § 12, we obtain the identity

$$\begin{vmatrix} a_1-a_3 & a_2-a_4 & a_3-a_5 \\ a_2-a_4 & a_1-a_5 & a_2-a_4 \\ a_3-a_5 & a_2-a_4 & a_1-a_3 \end{vmatrix} = - \begin{vmatrix} a_3-a_1 & a_4-a_2 & a_5-a_3 & \cdot & a_3-a_5 \\ & a_3-a_1 & a_4-a_2 & a_5-a_3 & \cdot \\ & & a_3-a_1 & a_4-a_2 & a_5-a_3 \\ & & & a_3-a_1 & a_4-a_2 \\ & & & & a_3-a_1 \end{vmatrix}.$$

The determinant form has the advantage of showing that the factor is further resolvable, for being centro-symmetric it is equal to

$$\begin{vmatrix} a_1-a_3+a_3-a_5 & a_2-a_4 \\ a_2-a_4+a_2-a_4 & a_1-a_5 \end{vmatrix} \cdot (a_1-a_3-a_3+a_5),$$

i.e., $$\{(a_1-a_5)^2-2(a_2-a_4)^2\}(a_1-2a_3+a_5):$$

so that

$$\begin{aligned} &C(a_1\ a_2\ a_3\ a_4\ a_5\ a_4\ a_3\ a_2) \\ &= (a_1+2a_2+2a_3+2a_4+a_5)\cdot(a_1-2a_2+2a_3-2a_4+a_5) \\ &\quad \cdot (a_1-2a_3+a_5)^2\cdot\{(a_1-a_5)^2-2(a_2-a_4)^2\}^2. \end{aligned}$$

15. Finally, it must be observed that, although it has been shown that

$$C(a_1, \cdots, a_{2n-1}) \div (a_1+\cdots+a_{2n-1})$$

and $$C(a_1, \cdots, a_{2n}) \div (a_1+\cdots+a_{2n})(a_1-a_2+\cdots-a_{2n})$$

are expressible as persymmetric determinants, which become zero-axial skew determinants when $a_2, \cdots, a_{2n-1}$ and $a_2, \cdots, a_{2n}$ are the same on being read backwards as on being read forwards, it has not been formally proved that all the factors of these persymmetric determinants can also be expressed as persymmetric determinants having the same property. This, however, would seem to be the case.

Note on the Reducing Power of the Living Animal Tissues. By David Fraser Harris, B.Sc. (Lond.), M.B., C.M.

(Read April 5, 1897.)

The following demonstration of the avidity for oxygen on the part of the living animal tissues was made incidentally while I was injecting the gelatine and soluble Berlin-blue mixture (Ranvier) into the blood-vessels of cats and rabbits in preparation of histological material. It is a fundamental fact that living cells must have oxygen continually supplied to them; the tissues crave oxygen, and will take it from any substance within their reach capable of yielding it (the inspiratory phase of the internal respiration). Normally the HbO_2 yields it—the gas reaching the cells *via* the lymph. The left external jugular was chosen, and opened the instant the chloroformed animal's heart ceased to beat: thus, though the animal was "dead" as a whole, its tissues were alive for varying periods thereafter.

The fluid traversed heart and lungs before reaching the aorta and arterial circulation. On cutting up the organs I thought the injection must have "failed" in certain organs, for the kidneys and liver were markedly pale—pale green—and in some of the divisions of the portal vein the gelatine seemed uncoloured or "bleached" white. The lungs were of a deep blue; and such organs as brain and eye were blue, while there was no loss of colour where the fluid and the blood had mixed in the vessels (in a case in which the blood had not been first washed out of the vascular system). Here we had clearly a case of de-oxidation of the deep blue ferric ferrocyanide (Berlin or Prussian blue) to the pale green or almost colourless ferrous ferrocyanide, which, however, underwent re-oxidation on exposure to the air, for the blue colour returned progressively in from ten minutes to twenty-four hours after. Next morning, indeed, organs that seemed empty of injection showed full capillary contents.

It is interesting that there was no reduction of the pigment on the part of the blood itself, and that the feeble metabolism in pul-

monary tissue expressed itself in an absence of any de-oxidative change of colour. Organs, such as brain and eye, in which, compared with kidney and liver, there must be a much less energetic metabolism, remained blue.

These observations corroborate experiments of Ehrlich, in which alizarine-blue and methylene-blue were injected, and found more or less bleached to chromogens ("Der Sauerstoffbedürfniss des organismus," 1885).

When one reads the statement, "the liver is the seat of vigorous oxidation," that is the converse aspect of the subject; it is the seat of great oxidation in so far as its own tissue is concerned, but of correspondingly great de-oxidation so far as the blood and materials in its lymph are concerned.

Hæmatoporphyrinuria and its Relations to the Origin of Urobilin.

By David Fraser Harris, B.Sc. (Lond.), M.B., C.M.

(Read April 5, 1897.)

The chief pigment of normal urine is known as urobilin (seeing that this name indicates a relation to "bile," "urochrome" would be a better term, not connoting any view as to genetic relationship). The name for the chromogen "urobilinogen" would become "urochromogen." Other pigments are or may be present in normal urine, *e.g.*, the indigo-blue pigment whose ancestor is intestinal indol, and "uroerythim" genetically related, it is believed, to skatol, while, according to some physiologists, traces of "pathological urobilin" (which might be better called para-urochrome) and even of hæmatoporphyrin are recognisable in healthy urine.

Certainly, in some morbid urines, urohæmatoporphyrin has been the chief pigment replacing urobilin, while, in a few very rare ones, a pigment allied to it, but, according to M'Munn, less deoxidised, has appeared. This substance, like urobilin and urohæmatoporphyrin, is a proteid-free, iron-free pigment, as yet unnamed. I propose to call it meio-de-oxyhæmatoporphyrin. Urobilin is amber-coloured, urohæmatoporphyrin makes the urine of orange tint, while the rarer ally gives it a deep port-wine colour: urines containing this last do not decompose for many weeks.

I lately encountered meio-de-oxyhæmatoporphyrin in the urine from a case of rare skin-disease, Dermatitis herpetiformis bullosa.* Both these hæmatoporphyrins have absorption-spectra resembling that of alkaline hæmatoporphyrin, and each, on the addition of strong sulphuric acid, is changed to the characteristic two-banded acid-hæmatoporphyrin.

In what way is urobilin related to these rarer and less deoxidised pigments? Let us dispose, in the first place, of the biliary theory of the formation of urobilin, which is, that bilirubin or biliverdin in the intestine is acted upon by nascent hydrogen,

* M'Call Anderson, *Scot. Med. and Sur. Jour.*, Feb. 1897.

and thereby reduced to a body resembling the artificial hydrobilirubin which, absorbed from the intestine, traverses the liver, and so, by the lungs, reaches the arterial stream for the kidney. Against this theory may be urged the following considerations:—

1. In animals with a biliary fistula the urine has urobilin.

2. In Copeman and Winston's case * of human biliary fistula, where the fæces were "clay-coloured," the urine contained urobilin, on some days in excessive quantity.

3. A body constantly present is made to depend upon a fluctuating quantity like "nascent hydrogen."

4. Why, if bilirubin, formed in the liver is at once excreted from it in the bile, should hydrobilirubin pass through the liver unchanged, or pass through it at all into the blood for the right auricle?

5. In fevers, whereas the amount of bile is diminished, urobilin is increased.

The fate of the bile-pigments will still further determine the site of the source of urobilin. The bile-pigments, acted upon in the intestine by some *de-oxidising* agent either contained in pancreatic juice † or acting in its presence, forms stercobilin, the brown fæcal pigment thus eliminated; for (1) before the pancreas secretes we have meconium (thick fœtal bile-pigment unaltered); and (2) although bile be secreted, if the pancreas is diseased or its duct occluded, stercobilin is absent from the faeces; (3) in vigorous catharsis, when bile-pigment is hurried through the intestine, it may be yellow (bilirubin) and not brown (stercobilin).

[Recently it has been suggested, from the spectroscopic affinities between stercobilin and pathological urobilin, that the presence of the latter in urine may be due to an abnormal absorption of the former from the bowel.]

Unquestionably hæmoglobin *via* hæmatin (normally) is the parent of both the bile-pigment and the urinary pigment, and it is most probable both are the offspring of hæmatin of the same generation.

Thus, though the BILIARY origin of urobilin must be given up, there is much to point to its *hepatic* origin; that the liver is the

* *Jour. of Phys.*, x. p 21.

† T. J. Walker, *Med.-Chir. Trans.*, Lond., vol. lxxii., 1890.

chief seat of the formation from hæmatin of the immediate antecedent of urobilin there can be no doubt, for—

(1) Increased destruction of red blood-discs in the liver, such as follows from injecting into the portal vein any substance which liberates HbO_2, leads to an increase of urinary pigment.

(2) In pernicious anæmia much iron is deposited in liver cells, and the urobilin is increased, indicating an exaggerated destruction of HbO_2; in such urines the band at F can be seen (without concentrating the urine), sometimes pathological urobilin, a more deoxidised pigment, is formed due to the abnormally intense deoxidative hepatic katabolism of HbO_2.

Since M'Munn made urobilin by oxidising acid-hæmatin by per-oxide of hydrogen, and then briefly de-oxidising with sodium-amalgam, he has suggested that "urobilin is formed by nascent oxygen in the tissues." As to the *modus operandi* of its formation there are, then, two theories: (1) The reduction theory; (2) The oxidation theory. Though most probably both reduction and oxidation occur in the formation of uroblin, neither is alone the method; and as it was unsatisfactory to attribute the reduction to intestinal nascent H, so it is unlikely that nascent oxygen in the tissues performs the oxidation. The living protoplasm of the tissues craves oxygen for itself, and fixes it interstitially. If the liver is the great source of urobilin formation, it is, as we should expect, pre-eminently a region of *de*-oxidations.

All other pigments are artificially produced by processes of more or less complete reduction, thus:—

(1) Hæmatin acted on by nascent H = Hæmatoporphyrin.

(2) Hæmatin (but not bilirubin) acted on by zinc + H_2SO_4 to full reduction, yields pathological urobilin, intermediate stages in this process giving "urobilinoidin" (le Nobel), the chromogen of urohæmatoporphyrin.

(3) Urobilin reduced yields pathological urobilin (para-urochrome).

From these facts and other considerations we may follow M'Munn in believing that the order of the pigments as to degree of de-oxidation is as under:—

(1) Oxy-Hæmoglobin.

(2) Hæmoglobin.

(3) Hæmatin.

(4) Hæmatoporphyrin.

Meio-de-oxyhæmatoporphyrin.

Urohæmatoporphyrin.

Urobilinoidin.

Urobilin.

Urobilinogen.

Para-urochrome (pathological urobilin).

Para-urochromogen.

We have then to account for processes of decomposition, oxidation, and reduction in the transformation of blood-pigment to urinary-pigment. I believe it may be done thus:—

Taking the liver in health, oxyhæmoglobin reduced is first decomposed to hæmatin by separation of the proteid globin; the hæmatin is, by separation of its iron, further decomposed into some antecedent of urobilin, perhaps its chromogen.

This substance, secreted "internally" by the liver, passes on to the heart, and thence to the lungs, where it becomes oxidised to urobilin (hence the urobilin band in blood-serum*), and is then swept on into the arterial stream, to be (some of it) rapidly reduced at the moment of renal excretion; hence we are prepared to find in urine *some* urobilin and *some* urobilinogen, which latter, as the urine stands, becomes oxidised to the pigment—a process hastened by the acid fermentation, or by the action of potassium permanganate, HCl or HNO_3 on the urine.

In pernicious anæmia the initial amount of this hepatic katabolism is increased, or is excessive in the degree of de-oxidation (as above indicated). On this view M'Munn's oxidation-stage by H_2O_2 has its analogy in the lungs, and his "brief reduction" by Na-Hg in the kidneys.

But there are probably other than hepatic sites of urobilin formation, and tissues other than the liver can to a certain extent transform hæmoglobin. The blood of old clots (cerebral, aneurysmal, or of corpora lutea) often shows crystals of a proteid-free, iron-free pigment hæmatoidin (identical in analysis with bilirubin); blood effused into the peritoneum and other sites causes an increase

* M'Munn, *Proc. Roy. Soc.*, 1881, p. 231.

of urobilin or pathological urobilin, which also occurs in acute muscular rheumatism and in other fevers.

[The presence of an increase of urobilin in febrile urine may possibly be due not only to an excessive initial production of pigment-antecedent normally oxidised to urobilin, but also to a diminished reduction to chromogen by the relatively inactive renal epithelium.]

Tissue, other than hepatic, can katabolise blood-pigment, but urobilin is not always the end-product—the initial reduction may not proceed so far as that. These other tissues may be included under three heads:—

I. muscles; II. skin; III. connective tissues, with bones, cartilages, etc. Now, it is of blood from these three systems, in addition to the hepatic supply, that the blood in the right auricle may be *chemically* viewed as constituted: blood from these four sources contributes to arterial and therefore renal blood.

Under certain diseased conditions of one or more of these three systems, urohæmatoporphyrin replaced urobilin in the urine, notably in acute rheumatism (muscular and articular), effusion of blood into the peritoneum, and Addison's disease; but the following is a complete list of M'Munn's cases,* with the seats of depraved metabolism indicated (as above numbered):—

Acute rheumatism (I. and III.)	
Pericarditis	(III.)
Peritonitis	
Meningitis	
Cirrhosis of liver	
Peritoneal blood effusion	
Croupous pneumonia.	
Typhoid fever	(II.)
Measles	
Addison's disease	
Hodgkin's disease.	
Sulphonal overdosing.†	

The less de-oxidised ally, meio-de-oxyhæmatoporphyrin, has appeared as *the* pigment on one or two occasions—twice in obscure

* *Journal of Physiology*, vol. x.

† Oswald, *Glas. Med. Journal*, 1895.

neurotic fatal cases in women, and once in a case with grave cutaneous lesion (reported above). It is noteworthy that in muscles we have a hæmoglobin derivative, myo-hæmatin, whose spectrum is very similar to urohæmatoporphyrin. Whatever else it is, it is a reduction product, for oxidation causes its bands to disappear. We might note the anæmia in Addison's and Hodgkin's diseases; and as to System II., that if it be *destroyed* over a pretty extensive area, as in a severe "burn," we have hæmoglobinuria—a paralysis of HbO_2-transforming power.

Normally, then, we may regard muscles, skin, and connective-tissues as seats of a subsidiary formation of urobilin from a hæmatin derivative, which, if it be excessively reduced, gives rise to pathological urobilin; if, owing to as yet most obscure deviations from healthy metabolism in these systems, it be only imperfectly reduced, we then have, in not a few cases, urohæmatoporphyrin, and in very rare ones its less reduced dark red ally. The degree of completeness of the initial reduction determines which of these hæmatin derivatives is to appear, for the amount of subsequent pulmonary oxidation and renal reduction is probably the same for all.

[In certain diseases this would, of course, not be so, as in pneumonia, where pulmonary oxidation is deficient, more, then, of the chromogen and less of the pigment from the system in which the prominent lesion existed would be excreted.]

A Contribution to the Comparative Anatomy of the Mammalian Organ of Jacobson. By R. Broom, M.D., B.Sc. *Communicated by* Sir William Turner.

(Read June 7, 1897.)

(*Abstract.*)

In this paper the author confines his attention to the relations of the organ, and especially to the structure of Jacobson's cartilage, the nasal floor cartilage, and the different developments to which they give rise. From the study of a large series of forms he has been led to the following general conclusions:—(1) That the organ is generally well developed in the primitive groups, and feebly developed in the higher forms; (2) that among allied animals the organ is, as a rule, better developed in the small species than in the large; (3) that in the various members of each Natural Order the organ is constructed on a common type, which admits of only very slight variation; and (4) that, even when the organ is quite absent in certain members of an Order, the nasal floor cartilages are constructed on the same type as in those members in which the organ is developed.

From the very slight tendency there is in the organ and its cartilaginous adjuncts to vary with changing habits, the author concludes that the anatomy of the region affords a valuable factor in classification. A study has been made in most of the Mammalian orders, and the relationships and inter-affinities of the different types have been critically examined.

In the Monotremata the organ is very well developed. The simple nasal floor cartilage in front gives rise behind to Jacobson's cartilage and a well-developed outer nasal floor cartilage, which latter passes inwards and supports Jacobson's cartilage. In this order Jacobson's cartilage differs from all other mammals, and resembles lizards in forming a well-marked turbinal process.

In the Marsupialia and Edentata the organ is much less de-

veloped, and the cartilages are more or less rudimentary. The nasal floor cartilage is simple in its construction, and gives rise behind to Jacobson's cartilage and a rudimentary outer nasal floor cartilage. There is no turbinal to Jacobson's cartilage, but there are rudimentary indications of its former existence in both groups. In the Marsupialia the organ almost invariably opens into the upper end of the naso-palatine canal; in the Edentata it opens into the nasal cavity considerably in advance of the canal.

In the Rodentia we have a condition which agrees considerably with the Edentata as regards Jacobson's cartilage and the mode of opening of the organ; but there is an additional feature present in a peculiar development of the outer nasal floor cartilage. This cartilage, which is quite unconnected with any other, forms a floor to part of the nasal cavity behind the naso-palatine canal, and passing forward forms a support to the outer wall of the canal.

In all the higher Eutheria the organ and its cartilages are formed on a common type, with very slight modifications. In this variety the peculiarity is due to Jacobson's organ opening into the naso-palatine canal, and to both Jacobson's cartilage and the outer nasal floor cartilage sending forward processes supporting the ducts. The organ itself is, as a rule, rudimentary,—at least, when compared with the lower mammals.

From the consideration of the peculiarities shown, the author proposes to divide the Eutheria into two groups,—the former in which the nasal floor cartilages are formed on the primitive type, and the other in which the cartilages undergo a peculiar and characteristic development. The former group, for which the name *Archæorhinata* is proposed, will include the Edentata, and, most probably, the Rodentia. The higher group, for which the name *Cœnorhinata* is proposed, will include the Chiroptera, Insectivora, Carnivora, and Ungulata, with most probably the Primates, and possibly the Cetacea and Sirenia.

Experiments on the Electrical Phenomena produced in Gases by Röntgen Rays, by Ultra Violet Light, and by Uranium.

By **Lord Kelvin**, G.C.V.O., F.R.S., etc. etc.; **J. Carruthers Beattie**, D.Sc., F.R.S.E., 1851 Exhibition Scholar, Research Fellow of the University of Glasgow; and **M. S. de Smolan**, Ph.D., Research Fellow of the University of Glasgow.

(Read December 21, 1896.)

Art. I.—Electrification of Air by Röntgen Rays.

§ 1. To test whether or not the Röntgen rays have any electrifying effect on air, the following arrangement was made.

A lead cylinder 76 cms. long, 23 cms. diameter, was constructed; and both ends were closed with paraffined cardboard, transparent to the Röntgen rays. Outside the end distant from the electrometer (see diagram 1) a Röntgen lamp * was placed. In the other end two holes were made, one in the middle, through which passed a glass tube (referred to below as suction pipe) of sufficient length to allow the end in the lead cylinder to be put into any desired place in the cylinder. By means of this, air was drawn through an electric filter † by an air pump. The other hole, at a little distance from the centre, contained a second glass tube by which air was drawn through india-rubber tubing from the open-air quadrangle outside the laboratory.

In one series of experiments the end of the suction pipe was kept in the axial line of the lead cylinder at various points 10 cms. apart, beginning with a point close to the end distant from the Röntgen lamp.

In every case the air drawn through the filter was found to be

* The Röntgen lamp was a vacuum vessel with an oblique platinum plate (Jackson pattern).

† Kelvin, Maclean, Galt, *Proc. Roy. Soc.*, London, March 14, 1895.

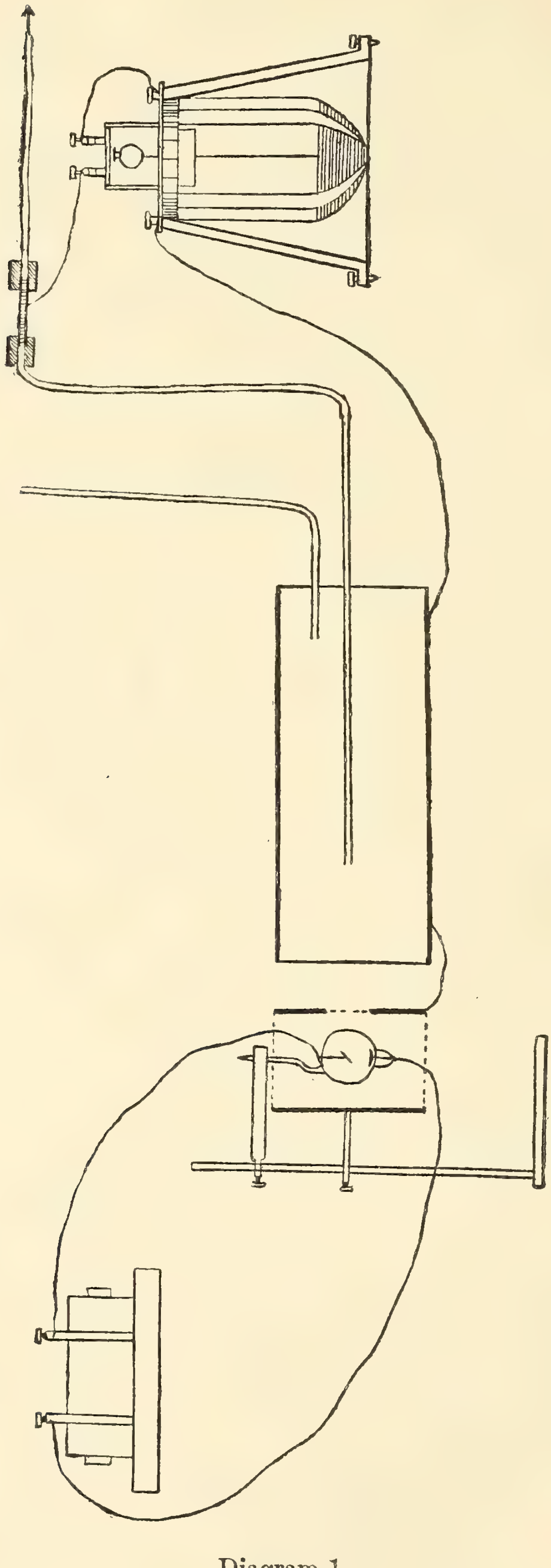


Diagram 1.

negatively electrified when no screen or an aluminium screen was interposed between the Röntgen lamp and the near end of the lead cylinder. The air was found not electrified at all, or very slightly negative, when a lead screen was interposed.

When the Röntgen lamp was removed or stopped, and air was still pumped through the filter, no deflection was observed on the electrometer. This proved that the air of the quadrangle was not electrified sufficiently to show any deflection when thus tested by filter and electrometer.

Similar results were obtained with the end of the suction pipe placed so as to touch the floor of the lead cylinder, or the roof, or the sides. Whether the air was pumped away from a place in the cylinder permeated, or from a place not permeated, by the Röntgen rays, it was in all cases found to be negatively electrified.

The following are some of the results obtained on December 16 and 17. The electrometer was so arranged as to give 140 scale divisions per volt.

Conditions.—Large lead cylinder metallically connected with sheath of electrometer. Röntgen lamp surrounded by a lead sheath, which latter was also connected to electrometer-sheath. There was a window in this lamp-sheath 2·5 cms. broad and 5 cms. high. This window could be screened by aluminium or by lead. These screens were always connected metallically to sheaths. During all the experiments a Bunsen lamp (not shown in the diagram) was kept constantly burning, with its flame about 30 cm. below the Röntgen lamp.

Results.—Röntgen lamp in action; air drawn from lowest point of end of lead cylinder next to the R. lamp.

December 16 :—

3.55 p.m.	− 61 scale divisions in 2 mins. with aluminium screen.				
,,	− 63	,,	,,	,,	no screen.
,,	− 14	,,	,,	,,	lead screen.
4.20 p.m.	Air drawn from point on lowest line of lead cylinder 26 cms. distant from R. L. end.				
,,	− 14 scale divisions in 2 mins. with lead screen.				
,,	− 78	,,	,,	,,	no screen.
,,	− 24	,,	,,	,,	lead screen.
,,	− 83	,,	,,	,,	alumin. screen.
,,	− 13	,,	,,	,,	lead screen.

Dec. 17.	R. L. acting, and air drawn through filter.			End of suction pipe kept in axial line of cylinder.
10.47 a.m.				cms.
– 44	in 2 mins.	with alumin.	screen . .	68 from R. L. end.
0	,,	,, lead	,, . .	68 ,, ,,
– 28	,,	,, no	,, . .	58 ,, ,,
– 24	,,	,, no	,, . .	48 ,, ,,
0	,,	,, lead	,, . .	48 ,, ,,
– 23	,,	,, alumin.	,, . .	48 ,, ,,
– 26	,,	,, alumin.	,, . .	38 ,, ,,
– 9	,,	,, lead	,, . .	38 ,, ,,
– 7	,,	,, lead	,, . .	28 ,, ,,
– 26	,,	,, alumin.	,, . .	28 ,, ,,
– 36	,,	,, alumin.	,, . .	18 ,, ,,
– 21	,,	,, alumin.	,, . .	8 ,, ,,

§ 2. We had previously made experiments with a sheet-iron funnel 1 metre long, 14·5 cms. diameter; and with a glass tube 150 cms. long, 3·5 cms. diameter; and with an aluminium tube 60 cms. long, 4·5 cms. diameter. Air was pumped from different parts while the Röntgen rays were shining along the tube from one end, which was closed by paraffined paper stretched across it. In every case the air was found to be negatively electrified.

In those earlier experiments the air drawn away was replaced by air coming in from the laboratory at the open end of the tube. We found evidence of disturbance due to electrification of air of the laboratory by brush discharges from electrodes between the induction coil and Röntgen lamp, and perhaps from circuit-break spark of induction coil. These sources of disturbance are eliminated by our later arrangement of lead cylinder covered with cardboard at both ends, as described above, and air drawn into it from open-air outside the laboratory.

§ 3. We have also found a very decided electrification of air—sometimes negative, sometimes positive—when the Röntgen rays are directed across a glass tube or an aluminium tube, through which air was drawn from the quadrangle outside the laboratory, to the filter.

A primary object of our experiments was to test whether air electrified positively or negatively lost its charge by the passage of Röntgen rays through it. We soon obtained an affirmative answer to this question, both for negative and positive electricity. We found that positively electrified air lost its positive electricity, and

in some cases acquired negative electricity, under the influence of Röntgen rays; and we were thus led to investigate the effect of Röntgen rays on air unelectrified to begin with.

§ 3A. The arrangement described in § 1 was again used to test whether or not air was electrified by ultra-violet rays. The ultra-violet rays were produced by an arc lamp. This lamp was placed about a cm. distant from the closed end of the large lead cylinder. The rays passed into the cylinder through a quartz window. The air in the cylinder from the immediate neighbourhood of this window was drawn through an electric filter. No effect was produced on the electrometer. An exactly corresponding arrangement—§ 1—with Röntgen rays gave negative electrification of the air.

Art. II.—On Apparent and Real Diselectrification of Solid Dielectrics Produced by Röntgen Rays and by Flame.

(Read February 15, 1897.)

§ 4. The fact that air is made conductive by flame, by ultra-violet light, by Röntgen rays, and by the presence of bodies at a white heat, has been shown by many experimenters. We propose in this communication to give some results bearing on this conductivity of air, based chiefly on experiments of our own.

§ 5. We have examined more particularly the behaviour of paraffin and of glass.

In our first experiments with paraffin we used a brass ball of about an inch diameter, connected to the insulated terminal of an electrometer by a thin copper wire soldered to the ball. The ball and the wire were both coated to the depth of about $\frac{1}{8}$th of an inch with paraffin. The ball was then laid on a block of paraffin in a lead box with an aluminium window, both of which were in metallic connection with the case of the electrometer. By this means we avoided all inductive effects.

The electrometer was so arranged as to read 140 scale divisions per volt.

After testing the insulation the paraffin ball was charged posi-

tively and the rays played on it. After two minutes the electrometer reading was steady at 0·5 of the initial reading. The electrometer was then discharged by metallic connection, and again charged positively. Its reading remained steady after three minutes at 0·63 of the initial charge. In the third and fourth experiments the readings after three minutes were ·81 and ·90 of the initial charges respectively.

The ball was next charged negatively. When the rays were played on it a steady reading was obtained after four minutes at ·18 of the initial charge. In the second, third, and fourth experiments the steady readings after four minutes were ·45, ·70, and ·78 of the initial charges respectively.

§ 6. The paraffin was then removed and the brass ball polished with emery paper; whether the charge was positive or negative, it fell in about five seconds to one definite position, 50 scale divisions on the positive side of the metallic zero, when the Röntgen rays were played on the charged ball.

§ 7. These experimental results demonstrate that the Röntgen rays *did not produce sensible conductance* between the brass ball, when it was coated with paraffin, and the surrounding metal sheath; and that *they did produce it* when there was only air and no paraffin between them. From experiments by J. J. Thomson, Righi, Minchin, Benoist and Hurmuzescu, Borgmann and Gerchun, and Röntgen,* we know that air is rendered temporarily conductive by Röntgen rays, and Röntgen's comparison of the effect of the rays with that of a flame shows that our experimental results are explained by the augmentation of the electrostatic capacity (quasi-condenser) of the brass ball by the outside surface of its coat of paraffin being put into conductive communication with the surrounding lead sheath and the connected metals.

§ 8. In our second experiments we have endeavoured to eliminate the influence of the varying capacity of this quasi-condenser. For this purpose, we placed a strip of metal connected to the

* J. J. Thomson, *Proceedings R.S.L.*, February 13, 1896; Righi, *Comptes Rendus*, February 17, 1896; Benoist and Hurmuzescu, *Comptes Rendus*, February 3, March 17, April 27, 1896; Borgmann and Gerchun, *Electrician*, February 14, 1896; Röntgen, *Würzburger Phys. Med. Gesellschaft*, March 9, 1896; Minchin, *Electrician*, March 27, 1896.

insulated terminal of the electrometer inside an aluminium cylinder; the space between the metal and the cylinder was first filled with air, afterwards with paraffin. The aluminium was connected to the case of the electrometer, and inductive disturbances were avoided by surrounding the copper wire connecting the metal to the isulated terminal with a lead sheath in metallic connection with the electrometer sheath (see diagram 2).

In our first experiments with this apparatus we had air, instead of the main mass of paraffin, separating the insulated metal from the surrounding aluminium tube, as shown in the diagram, and we had only small discs of paraffin serving as insulating supports for the ends of the metal, and not played on by the Röntgen rays. When the metal thus supported was charged, whether positively or negatively, the Röntgen rays diselectrified it in about five seconds; not, however, to the metallic zero of the electrometer, but to a "rays-zero" depending on the nature of the insulated metal surrounding it.

With paraffin between the aluminium cylinder and the insulated metal within, as shown in the diagram, the following results were obtained:—

December 30, 1896. 5.30 P.M.—Interior metal charged negatively. Total charge, 356.

Röntgen lamp in action and no screen,	39 scale divisions	discharged	in 5	mins.
R. L. not acting, ...	25	,,	5	,,
R. L. again acting and no screen, .	17	,,	5	,,

5.45.—Interior metal charged positively. Total charge, 244.

R. L. in action and lead screen, .	1 scale division	discharged	in 3	mins.
R. L. in action and no screen, .	6	,,	3	,,
R. L. not acting, ...	0	,,	3	,,

Dec. 31, 1896. 10.54 A.M.—Interior metal charged positively. Total charge, 163.

R. L. not acting, ...	2 scale divisions	discharged	in 3	mins.
R. L. acting and no screen, . .	1	,,	3	,,
11.0—R. L. stopped, ...	1·5	,,	2	,,
R. L. again acting, no screen, .	3	,,	2	,,
R. L. stopped, ...	2·5	,,	3	,,

11.12.—Interior metal charged negatively. Total charge, 342.

R. L. not acting, ...	10 scale divisions	discharged	in 3	mins.
R. L. acting, no screen, . .	21	,,	3	,,
11.18—R. L. stopped, . .	11·5	,,	3	,,
R. L. acting, no screen, . . .	16·5	,,	3	,,

These results are quite in accordance with those found in similar experiments by Röntgen; and they show that if paraffin is made conductive, it is only to so small an extent that it is scarcely perceptible by the method we have used.

§ 9. To make a similar series of experiments with glass, we used a piece of glass tubing 9·5 mm. in diameter, length 70 c.m., and 1 c.m. external diameter. The inside of this tube was coated with

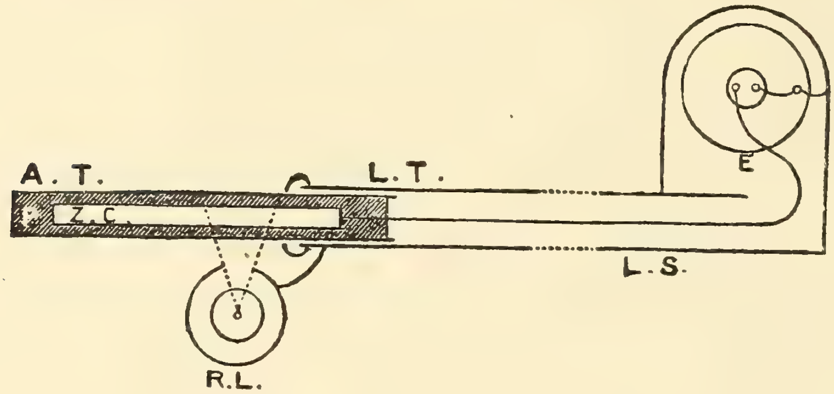


Diagram 2.—A.T, Aluminium tube; L.T, Lead tube; R.L, Röntgen lamp; L.S, Lead sheaths; E, Electrometer; P, Paraffin; Z.C, Zinc cylinder.

a deposit of silver, which was placed in metallic connection with the insulated terminal of the electrometer. The outside of the glass was covered with wet blotting-paper connected to sheaths.

With this arrangement we obtained the following results :—

Feb. 8, 1897.—Insulated terminal of electrometer charged to − 333 scale divisions from the metallic zero.

4.23.—Röntgen lamp, acting,	0·5 sc. div. lost in	3 mins.
,, ,, not acting,	1·0 ,, ,,	5 ,,

Charge to + 164 scale divisions from the metallic zero.

4.36.—Röntgen lamp, not acting,	13 sc. div. lost in	7 mins.
,, ,, acting,	8·5 ,, ,,	5 ,,
,, ,, not acting,	6·0 ,, ,,	6 ,,
,, ,, acting,	3·5 ,, ,,	5 ,,
,, ,, not acting,	3·5 ,, ,,	5 ,,

[Sensibility of electrometer, 140 scale divisions per volt.]

We next removed a part of the wet blotting-paper from the outside of the glass, and, after having charged the insulated interior metal deposited on the inside of the glass, we heated the exposed part with a spirit flame, in this way making the glass a conductor. Thus with a charge of + 280 scale divisions from the

metallic zero, the loss in 30 seconds, during which time the glass was heated in the spirit flame, was 90 scale divisions; in the next minute, with no further heating, the loss was 20 scale divisions. Reapplication of heat gave complete discharge in $2\frac{1}{2}$ minutes. Thus we see that our method is amply sensitive to the conductance produced in glass by heating.

We conclude that the Röntgen rays do not produce any conductance perceptible in the mode of experimenting which we have hitherto followed.

§ 10. A similarity in effects produced by flame and by Röntgen rays is brought out by the following experiments.

Two similar sticks of paraffin, which we shall call A and B respectively, each of about 4 sq. cm. cross section, were coated throughout half their lengths with tinfoil. These tinfoils ought to be each metallically connected to sheaths.

To obtain a sufficiently delicate test for their electric state, a metal disc of 3 cm. diameter was fixed horizontally to the insulated terminal of the electrometer.

The two pieces of paraffin were first diselectrified by being held separately in the flame of a spirit-lamp. Their non-tinfoiled ends were then pressed together, and their electric state again tested after separation. It was found that they were still free from electric charge. After this B was charged by being held over the pointed electrode of an inductive electric machine. The quantity of electricity given to it in this way was roughly measured by noting the electrometer reading when the paraffin was held at a distance of 4 cm. above the metal disc connected to the insulated terminal of the electrometer.

The free ends of A and B were again held together, and, after separation, both pieces were tested separately. The charged one, B, had suffered no appreciable loss, and the other, A, induced an electrometer reading of a few scale divisions in the same direction, when held as near as possible to the metal disc without touching it. This showed that an exceedingly minute quantity of electricity had passed from B to A when they were in contact.

A was then diselectrified by being held alone in the flame. The ends of A and B were again put together, and in this position were passed through the flame. They were tested with their

ends still pressed together, and it was found that when held as near as possible to the metallic disc without touching it, no reading was produced on the electrometer. After this they were separated and tested separately; and it was found that B, when held over the disc, gave a large reading in the same direction as before it had been passed through the flame, and A (which was previously non-electrified) gave a reading of about the same amount in the opposite direction.

The same results were obtained when Röntgen rays were substituted for the flame.

The explanation clearly is this: the flame or the Röntgen rays put the outer paraffin surfaces of A and B temporarily in conductive communication with the tinfoils, but left the end of B, pressed as it was against the end of A, with its charge undisturbed. This charge induced an equal quantity of the opposite electricity on the outer surfaces of the paraffin of A and B between the tinfoils; half on A, half on B.

When the application of flame or rays was stopped, this electrification of the outer paraffin surfaces became fixed. B, presented to the electrometer, showed the effect of the charge initially given to its end, and an induced opposite charge of half its amount on the sides between the end and the tinfoil. A showed on the electrometer only the effect of its half of the whole opposite charge induced on the sides by the charge on B's end.

We have here another proof that paraffin is not rendered largely conductive by the Röntgen rays. Had it been made so, then the charge given to the end would have leaked through the body of the paraffin to the outside, and have been carried away either by the tinfoil or by the conductive air surrounding the non-tinfoiled parts.

To show that the induced charges were fixed on the sides, the two sticks, A and B, were next coated with tinfoil throughout their whole length, only one end of each being uncovered. The uncoated end of B was then charged and pressed against that of A, and the two were held either in the flame of a spirit-lamp or in the Röntgen rays. When taken out of the flame or the Röntgen rays, and then separated and tested separately, it was found that

B had retained its charge practically undiminished, and that A had acquired a very slight charge of the opposite kind.

§ 11. Instead of placing the two ends of the paraffin in immediate contact, four pieces of metal of $\frac{1}{10}$ of a mm. thickness were placed one at each corner of one of the ends, so that when the sticks of paraffin were placed end to end there was now an air space of $\frac{1}{10}$ of a mm. between the paraffin ends. When B was charged and A not charged, and the two put end to end, and then exposed to flame or to Röntgen rays, it was found that B's end still retained its charge, and A's end acquired a very slight opposite charge.

With an air space of $\frac{1}{5}$ of a mm. the same results were obtained.

With the air space increased to 1 mm. the charge on B was less after the two had been passed through the flame or the rays.

§ 12. Similar experiments were made with rods of glass and of ebonite, with similar results.

Art. III.—On the Influence of Röntgen Rays in respect to Electric Conduction through Air, Paraffin, and Glass.

(Read March 1, 1897.)

§ 13. We have in §§ 5 to 10 described experiments respecting electric conduction through air, paraffin, or glass, when Röntgen rays fall on metal surrounded by air, paraffin, or glass, and positively or negatively electrified to potentials of two or three volts. We found that although air is rendered conductive, paraffin and glass are not rendered sensibly conductive when the differences of potential concerned are not more than two or three volts per centimetre of air, or per centimetre of paraffin, or per half-millimetre of glass.

We have now to describe an extension of the investigation to much higher voltages, in which we use an arrangement of two (quasi) Leyden jars, A and B, with their inside coatings connected together. The outside coating of A was connected to sheaths, the outside of B to the insulated terminal of the electro-

meter. In all the experiments to be described, B remained the same.

It consisted of a cylindrical lead can, 25 cms. long, 4 cms. diameter. A metal bar about 1 cm. diameter, 25 cms. long, was supported centrally on paraffin filling the whole space between it and the containing lead. This metal bar was connected by a wire to the internal coating of A. To protect this wire from inductive effects, it was surrounded by a tube of lead connected to sheaths.

The Leyden A, which was placed opposite the Röntgen lamp, was different according as we were experimenting on the discharge through air, through paraffin, or through glass.

To get a definite difference of potential, the two pairs of quadrants of the electrometer were first placed in metallic connection. Then one terminal of a battery or of an electrostatic inductive machine was connected to the internal coatings of the jars, and the other terminal to sheaths. The difference of potential produced was measured by a multicellular voltmeter in the case of differences under 500 volts, and on a vertical single vane voltmeter for higher differences.

When the desired difference of potential had been established, the metallic connection of the battery or electric machine with the internal coatings of A and B was broken, and this charged body left to itself. To find the loss due to imperfect insulation, the pair of quadrants in metallic connection with the outside coating of B was insulated in the ordinary way, and the deviation of the electrometer reading from the metallic zero per half-minute was observed. To find the loss when the rays were acting, the two pairs of quadrants were again placed in metallic connection, the Röntgen lamp set a-going, then the pair of quadrants connected to the outside coating of B was insulated from the other pair, and the deviation from metallic zero again observed per half-minute.

§ 14. In the experiments with air, the Leyden A consisted of an aluminium cylinder, 16 cms. long, 3 cms. in diameter. This cylinder projected beyond the left tube, and was connected to sheaths. The insulated metal inside it, which was a flat strip of aluminium, about 10 cms. long and $1\frac{1}{2}$ cms. wide, cut from the

same sheet as the surrounding aluminium tube, was supported at one end by a small piece of paraffin so placed as to be out of reach of the action of the Röntgen lamp. The rays from the lamp were allowed to pass from a lead cylinder surrounding it by a small hole about ·3 of a square cm. in area. They fell on the aluminium sheath transparent to them, and rendered the air between it and the insulated aluminium within conductive.

We tried various differences of potential, ranging from a few volts to 2200 volts. In one series of experiments we charged the insulated metal to − 97·5 volts, and then disconnected the battery electrodes. The lamp was then set a-going, and the electrometer deviation taken each half-minute for a minute and a half with one pair of quadrants insulated. The rays were then stopped, the quadrants metallically connected, and metallic zero again found. Then the reading during another period of one and a half minutes, with the rays acting, was observed, and so on until no deviation from the metallic zero of the electrometer was found with one pair of quadrants insulated, and the rays falling on the aluminium outside coating of the Leyden A. The sensibly complete discharge thus observed took place in about a quarter of an hour. We found that the rate of deviation from the metallic zero was the same as the difference of potential fell from − 97·5 volts to about − 4 volts. With differences of potential of − 930, − 1750, and − 2000 volts the rate of deviation was not appreciably greater than with ± 20 volts.

This confirms and extends, through a very wide range of voltage, the interesting and important discovery announced by J. J. Thomson and M'Clelland, in their paper in the Cambridge Philosophical Society *Proceedings* of March 1896, to the effect that the conduction of electricity through air under influence of the Röntgen rays is almost independent of the electric pressure when it exceeds a few volts per centimetre.

§ 15. In the experiments on paraffin, the outside coating of the Leyden A consisted of an aluminium cylinder 27 cms. long, 4 cms. diameter, connected to sheaths. A metal bar about 1·73 cms. in diameter, and 30 cms. long, supported centrally on paraffin filling the whole space between it and the aluminium sheath, constituted the inside coating. With this arrangement we made experiments with differences of potential of ± 94, ± 119, ± 238, − 2000, + 2500,

and − 2400 volts. At none of these potentials did we find any perceptible increase of conductance produced by the Röntgen rays above the natural conductance of the paraffin when undisturbed by them.

§ 16. In the experiments with glass, the Leyden A consisted of a glass tube silvered on the inside. The inside silvering was placed in metallic connection with the inside coating of B. That part of the glass tube which projected beyond the lead sheath was covered with wet blotting-paper connected to the sheaths. We observed the behaviour of glass under the Röntgen rays at differences of potential of + 800, + 1500, + 2000 volts. We found no indication of increased conductance due to the rays at these voltages.

We are forced to conclude that the experiments described by J. J. Thomson and M'Clelland do not prove any conductance to be induced in paraffin or glass by the Röntgen rays. It seems to us probable that the results described in their paper—pages 7 and 8—are to be explained by electrifications induced on surfaces of glass or of paraffin in contact with air rendered temporarily conductive by the Röntgen rays. (See § 7.)

ART. IV.—ON THE CONDUCTIVE EFFECT PRODUCED IN AIR BY RÖNTGEN RAYS AND BY ULTRA-VIOLET LIGHT.

(Read February 1, 1897.)

§ 17. We propose next to describe results of experiments on the electrical effects of Röntgen rays and of ultra-violet light when shone on metals, or through air between two metals mutually insulated; and electrified to begin with, by previously producing a difference of potentials between platinum electrodes of an electrometer metallically connected with them. In some of our experiments this potential-difference was zero, and the initial ± electrifications of the opposed surfaces depended solely on difference of volta-electric quality between their opposed surfaces.

§ 18. To investigate the effects of Röntgen rays, a hollow cylinder of unpolished aluminium connected to the electrometer sheaths was used. Along the axis of this a metallic bar was placed,

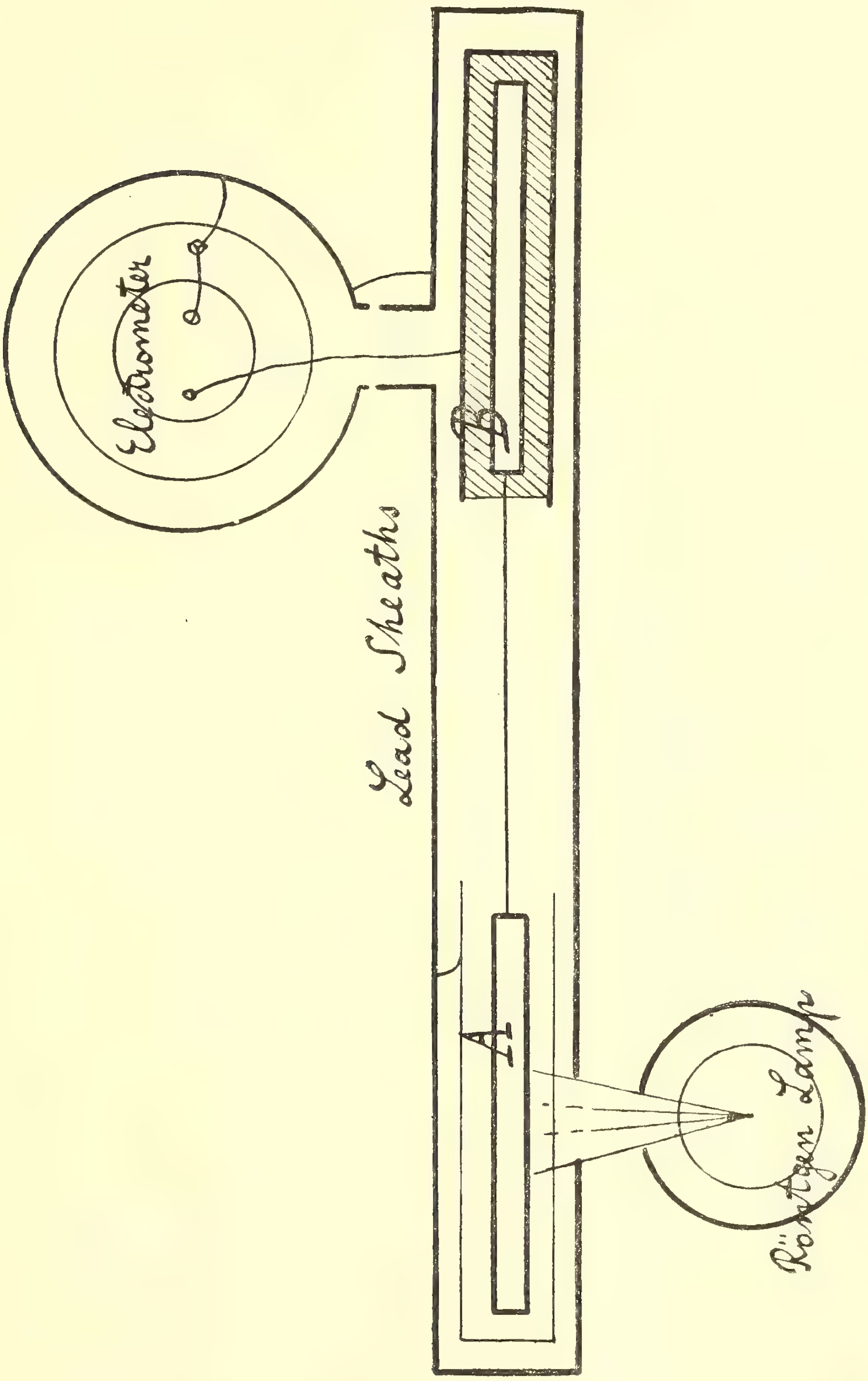


Diagram 3.

supported by its ends on small blocks of paraffin so situated as not to be shone on by the Röntgen rays. This insulated metal was connected by a copper wire to the insulated terminal of the electrometer. To protect it from inductive effects it was enclosed in a lead tube connected to the other terminal and to sheaths (see diagram 4).

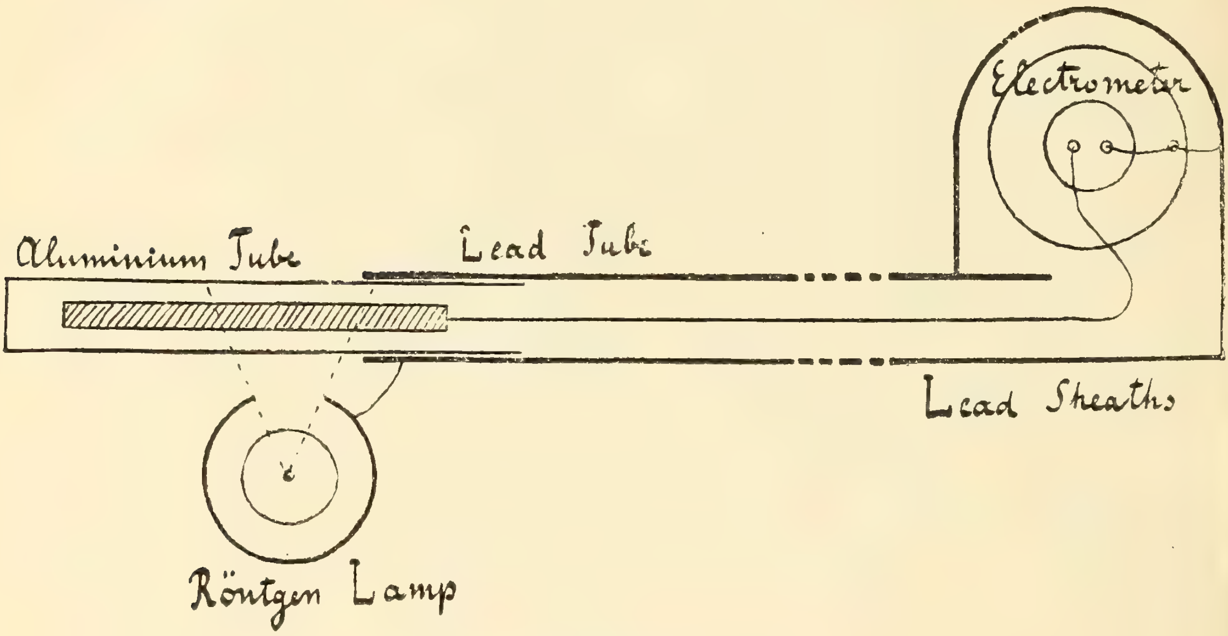


Diagram 4.

The Röntgen lamp was placed in a lead cylinder connected to sheaths. The rays passed into the tube of aluminium through a window in the lead cylinder, which could be screened or unscreened at will, as described in § 1.

The course of the experiment was the same with each insulated metal. The metal was charged first positively, then negatively; the Röntgen rays were then shone on it through the aluminium cylinder surrounding it, and the electrometer readings taken at fixed intervals, until a steady reading on the electrometer was obtained. The point at which the electrometer reading remained steady with the rays acting we shall call the *rays-zero.*

Finally, the insulated metal was discharged by metallic connection in the electrometer, and re-insulated; the rays were again shone on it until the rays-zero was again reached.

The following figures, taken from the laboratory book, show the effect obtained in this way when the insulated metal was amalgamated zinc.

The zero with the electrometer quadrants in metallic connection we shall afterwards speak of as the *metallic zero.*

December 31, 1896. 5.56 p.m.—Readings with one pair of electrometer quadrants insulated, and with Röntgen lamp acting.

– 72 scale divisions	from	metallic zero	after	5	secs.
– 87 ,,	,,	,,	,,	10	,,
– 91 ,,	,,	,,	,,	15	,,
– 92 ,,	,,	,,	,,	30	,,
– 93 ,,	,,	,,	,,	2	mins.

Afterwards steady.

Thus the difference between the rays-zero and the metallic zero is in this case – 93 scale divisions, or – 0·66 of a volt.

[Sensibility of electrometer 140 divs. per volt.]

This deviation from the metallic zero was not stopped by placing an aluminium screen over the window of the lead cylinder; on the other hand, it was stopped if a lead screen was used. If a positive or a negative charge was given to the insulated metal, and the Röntgen rays were shone through the aluminium cylinder surrounding it, the discharge went on till the rays-zero was reached; only then was the electrometer reading steady.

In the following table, Column II. gives the potential differences of the rays-zero from the metallic zero for twelve different metals insulated within the unpolished aluminium cylinder as described above. Column III. gives the differences for two of the same metals in the interior, but with the surrounding aluminium cylinder altered by polishing its inner surface with emery paper.

I. Insulated metal.	II.	III.
Magnesium tape	– 0·671 of a volt	
Amalgamated zinc	– 0·66 ,, ,,	
Polished aluminium	– 0·465 ,, ,,	
Polished zinc	– 0·343 ,, ,,	
Unpolished aluminium	– 0·349 ,, ,,	+ 0·35 of a volt.
Polished lead	– 0·257 ,, ,,	
Polished copper	+ 0·129 ,, ,,	
Polished iron nail	+ 0·182 ,, ,,	
Palladium wire	+ 0·255 ,, ,,	
Gold wire	+ 0·264 ,, ,,	+ 0·930 of a volt.
Carbon	+ 0·429 ,, ,,	

It is to be noted that the preceding experiments tell us in-

sufficiently as to what would happen had we shone the rays on an insulated metal surrounded by an absolutely identical metallic surface connected to sheaths. Another experiment towards answering this question will be described in a later part of our paper.

The preceding results of the action of Röntgen rays are very similar to, and wholly in accordance with, the results found by Mr Erskine Murray, and described by him in a communication to the Royal Society of London, March 19, 1896.

§ 19. They are analogous to those found for ultra-violet light by Righi (*Rend. R. Acc. dei Lincei*, 1888, 1889); Hallwachs (*Wiedemann's Annalen*, 34, 1888); Elster and Geitel (*Wiedemann's Annalen*, 38, 41, 1888); Branly (*Comptes rendus*, 1888, 1890), and others.

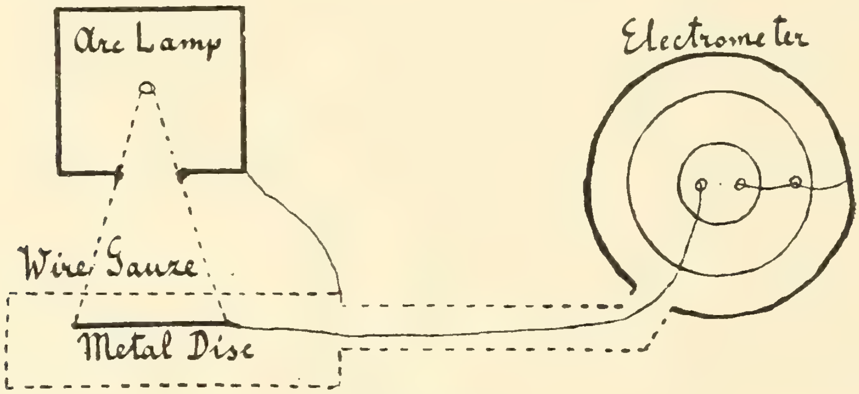


Diagram 5.

We have also made some experiments with ultra-violet light, in which this similarity is further brought out. The method we have employed is that of Righi.

A cage of brass wire gauze was made and connected to sheaths. Inside it the insulated metal was placed on a block of paraffin, and connected to the insulated terminal of the electrometer by a thin copper wire protected against inductive effects. The light from an arc lamp was then shone through the gauze, so as to fall on the insulated metal perpendicular to its surface (see diagram 5).

The experiments were of the same nature as those with the Röntgen rays, except that wire gauze letting through the ultra-violet light was substituted for the non-perforated aluminium cylinder transparent to the Röntgen rays. The insulated metal disc was 2 cms. distant from the gauze of brass wire. The steady

electrometer readings after the two pairs of quadrants were insulated and the ultra-violet light shining (which we shall hereafter refer to as the *ultra-violet-light-zero*) was observed.

The insulated metal was afterwards charged positively, and then negatively. The rate of discharge was observed until the ultra-violet-light-zero was reached.

With polished zinc as the insulated metal the following results were obtained.

The insulation was first tested. When no ultra-violet light was used it was found that the electrometer reading remained the same whether the two pairs of quadrants were in metallic connection or not. With the ultra-violet light shining the reading with the quadrants in metallic connection was the same as before, the readings with the quadrants disconnected were :—

January 14. 3h. 41m. p.m.

– 25 sc. divs.	from	metallic zero	after	15 secs.
– 45	,,	,,	,,	30 ,,
– 59	,,	,,	,,	45 ,,
– 67	,,	,,	,,	1 min.
– 80	,,	,,	,,	1½ ,,
– 89	,,	,,	,,	2 ,,
– 99	,,	,,	,,	3 ,,
– 101	,,	,,	,,	4 ,,

Afterwards steady.

[Sensibility of electrometer, 140 sc. divs. per volt.]

The difference thus found, between the metallic zero and the ultra-violet-light-zero, is – 101 or – 0·72 of a volt.

3h. 47m. Zinc charged positively to 219 scale divisions from the metallic zero.

Reading from metallic zero with ultra-violet light shining :—

	Time.
+ 124 . . .	after 15 secs.
+ 64 . . .	,, 30 ,,
+ 23 . . .	,, 45 ,,
– 13 . . .	,, 1 min.
– 55 . . .	,, 1½ ,,
– 79 . . .	,, 2 ,,
– 93 . . .	,, 2½ ,,
– 100 . . .	,, 3½ ,,
– 103 . . .	,, 4 ,,

Afterwards steady.

3h. 55m. Zinc charged negatively to 238 scale divisions from metallic zero :—

– 177 sc. divs.	from	metallic	zero after	15 secs.
– 149	,,	,,	,,	30 ,,
– 132	,,	,,	,,	45 ,,
– 124	,,	,,	,,	1 min.
– 113	,,	,,	,,	2 ,,
– 111	,,	,,	,,	3 ,,

Afterwards steady.

The following table shows the steady potential differences in the electrometer due to the conductive effect of ultra-violet light in our apparatus between the brass wire gauze and plates of various other metals.

Insulated metal :—

Polished zinc . . .	– 0·75 of a volt.
Polished aluminium . .	– 0·66 ,,
German silver . .	– 0·19 ,,
Gilded brass . . .	+ 0·04 ,,
Polished copper . .	+ 0·12 ,,
Oxidised copper . .	+ 1·02 ,,

The copper was oxidised by being held in a Bunsen flame.

In the case of polished zinc, polished aluminium, polished copper, and oxidised copper, both positive and negative charges were discharged at the same rate, if we reckon the charge of the insulated metal from its ultra-violet-light-zero. The rates of reaching the ultra-violet-light-zero were not observed for gilded brass and german silver.

It must again be noticed that our experiments do not tell us what would happen if an insulated metal, shone on by ultra-violet light, were surrounded by a metal of precisely the same quality of surface connected to sheaths.

§ 20. So far we have mentioned only experiments in which the rays, whether Röntgen or ultra-violet, fell perpendicularly on the insulated metal. We have also made some experiments with the rays going parallel to the metal surfaces.

For this purpose a cardboard box 46 cms. long, 19 cms. square (see diagram 6), lined, in the first instance, with tinfoil, connected to sheaths, was used. Inside this box an insulated disc of oxidised copper of 10 cms. diameter was supported in such a way as to

allow of its being fixed at different distances from the tinfoil-coated end-wall of the box facing it.

The distance between the disc and the tinfoil was at first 4 cms. The arc lamp was distant about 20 cms. from the box. The light from it shone through a slit in the tinfoil covering the side of the box perpendicular to the surface of the oxidised copper. The slit was 4 cms. long, 1 cm. broad. Its length was first placed parallel to the copper surface, so that the light admitted by it shone in the space between the two metals in such a way as not to illuminate either directly. It was found (1) that the ultra-violet-light-zero did not deviate from the metallic zero when the sheet of light passed between the two metals; (2) that a negative charge given to the insulated oxidised copper was not discharged; and (3) that

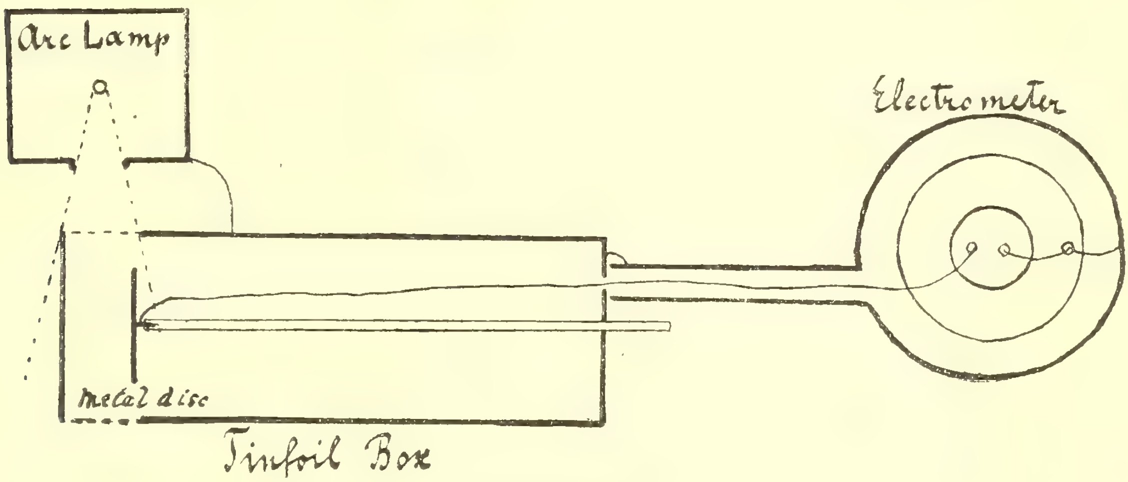


Diagram 6.

a positive charge was removed very slowly—about 4 scale divisions per minute from a charge of 197 scale divisions from the metallic zero.

When the length of the slit was placed perpendicular to the surface, so that a small portion of both metals, as well as the intervening air, was illuminated, it was found that the reading deviated about + 1 scale division per minute from the metallic zero. The oxidised copper was charged positively and negatively. Discharge took place at about 4 scale divisions per minute from a charge of + 202 scale divisions; and 3 scale divisions per minute from a charge − 246 scale divisions: the charge reckoned from the metallic zero in each case.

The slit was then so arranged as to allow the light to shine on

the oxidised copper alone. In this case the deflection went towards an ultra-violet-light-zero at about + 6 scale divisions per minute; and both positive and negative charges were discharged, the negative much more quickly than the positive.

§ 21. The ultra-violet light was now shone between the oxidised copper and the disinsulated tinfoil wall opposite to it, parallel to their surfaces so as to illuminate both. The difference between the metallic zero and the ultra-violet-light-zero was found to depend on the distance between the two surfaces. This will be seen from the following table:—

Jan. 28.	Ultra-violet-light-zero.			Distance between surfaces.	Time required to come to steady reading.
12.20 p.m.	+150	sc. divs. from metallic zero		4·3 cms.	4 mins.
2.0 ,,	+134	,,	,,	3·0 ,,	9 ,,
2.10 ,,	+121	,,	,,	2·0 ,,	5 ,,
2.20 ,,	+102	,,	,,	1·0 ,,	5 ,,
2.40 ,,	+ 86	,,	,,	0·6 ,,	5 ,,
2.50 ,,	+169	,,	,,	4·0 ,,	10 ,,
3.0 ,,	+161	,,	,,	5·0 ,,	5 ,,
3.20 ,,	+199	,,	,,	7·0 ,,	5 ,,

[Sensibility of electrometer 140 sc. divs. per volt.]

The fact that in experiments (2) and (6) a longer time was required before a steady reading was obtained, probably depended on the way the light fell on the surface and on variations in intensity of the light.

In this table we see that the steady electrometer reading (which we have called the ultra-violet-light-zero) is largely influenced by the distance between the plates, being greater the greater the distance. This is a very remarkable result. It was first discovered by Righi, and very clearly described in papers of his to which we have referred. It may be contrasted with the non-difference of electrometer readings for different distances between the plates in a volta-zinc-copper and single fluid cell.

§ 22. [*Added February* 6.—We have also made an exactly similar series of experiments with Röntgen rays. The same insulated oxidised copper plate was placed inside the same tinfoil box, and the Röntgen rays shone in between the two metals so as to shine on both. The following results were obtained with the oxidised copper at different distances:—

Feb. 5. 11.30 a.m.

Rays-zero.	Distance between surfaces.
+23·5 sc. divs. from metallic zero . .	1·2 cms.
+25·0 ,, ,, ,, . .	2·2 ,,
+23·0 ,, ,, ,, . .	3·8 ,,
+23·0 ,, ,, ,, . .	6·0 ,,

We next removed the oxidised copper plate, and substituted a polished zinc disc. With it we obtained the following results :—

Rays-zero.	Distance between surfaces.
−82 sc. divs. from metallic zero . .	1 cm.
−79 ,, ,, ,, . . .	1·5 ,,
−81 ,, ,, ,, . . .	3·0 ,,
−90 ,, ,, ,, . . .	7·0 ,,
−90 ,, ,, ,, . . .	7·5 ,,

The steady reading of the rays-zero was very nearly reached in each case in about 15 secs., but the observation was continued for one or two minutes till we found the reading steady.

Thus we see that, as previously found by Mr Erskine Murray, the rays-zero is independent, or nearly independent, of the distance between the opposed metallic surfaces.]

§ 23. Towards realising the case of an insulated metal surrounded by metal of identical surface-quality connected to sheaths, we covered over the oxidised copper with tinfoil. The tinfoil wall facing it was very rough, and not so well polished. The insulated tinfoil was 4 cms. distant from the end of the box to which its surface was parallel.

When the ultra-violet light fell on the insulated metal alone through a slit, the ultra-violet-light-zero was +53 scale divisions from the metallic zero. A charge given to it, whether positive or negative, was discharged slowly. After making these experiments, we again observed the difference of zeros, and found that now the ultra-violet light reading was at the end of the first four minutes +2 scale divisions from the metallic zero; at the end of the next four minutes it was −8 scale divisions from it.

When the ultra-violet light fell on the disinsulated metal and not on the insulated, the insulated when charged retained its charge.

With the light shining on both through a window 7 cms. broad, 13 cms. high, both positive and negative charges given to the insulated metal were discharged, and the ultra-violet light deviated from the metallic zero by – 152 scale divisions.

This difference was reduced to about – 30 scale divisions when the experiments were repeated after the apparatus had been left to itself for a night.

§ 24. To make similar experiments with the Röntgen rays, it was found necessary to cover the window near the lamp with tinfoil gauze connected to sheaths, and the window on the opposite side was covered with non-perforated tinfoil. In this way direct electrostatic induction was avoided. We had also a thin sheet aluminium window between the tinfoil gauze and the Röntgen lamp.

When the Röntgen rays fell on both insulated and disinsulated metal the rays-zero was – 5 scale divisions from the metallic zero, and both positive and negative charges fell to this zero in a few seconds.

With the rays shining only on the insulated metal the same small difference of zeros was obtained, and both positive and negative charges fell to the rays-zero, though much more slowly than before, in about four minutes.

With the Röntgen rays shining on the insulated tinfoil through the disinsulated tinfoil gauze, the rays-zero was – 9 scale divisions from the metallic zero, and both positive and negative charges were removed in about a minute.

On substituting an aluminium gauze for the tinfoil gauze, and sending rays through it on the insulated tinfoil, the rays-zero was + 25 scale divisions from the metallic zero.

[*Added February* 6.—With a polished zinc disc as the insulated metal, and with the same windows to the tinfoil box, the Röntgen rays were shed in between the insulated zinc and the opposite wall of tinfoil from a slit in a lead screen outside. This slit was 4 cms. long by 1 cm. broad. The distance between the two metals was 7 cms. The rays illuminated only part of the air space between the two, and also a part of the tinfoil covering the two windows.

The following are some of the results obtained :—

[Sensibility of electrometer 140 sc. divs. per volt.]

February 5, 1897. Zinc charged negatively to 285 scale divisions from the metallic zero.

Reading from metallic zero with Röntgen lamp acting :—

	Time.
− 276 scale divisions . .	after 1 min.
− 265 ,, . .	,, 2 ,,
− 255 ,, . .	,, 3 ,,
− 243 ,, . .	,, 4 ,,
− 227 ,, . .	,, 5 ,,
− 214 ,, . .	,, 6 ,,
− 184 ,, . .	,, 8 ,,

Discharge still continued.

The zinc was then discharged by metallic connection. The readings, with the Röntgen light shining, and the two pairs of electrometer quadrants again disconnected, were :—

− 4 sc. divs.	from metallic	zero after	½ min.
− 13 ,,	,,	,,	1½ ,,
− 41 ,,	,,	,,	2½ ,,
− 53·5 ,,	,,	,,	3½ ,,
− 61 ,,	,,	,,	4½ ,,
− 67 ,,	,,	,,	5½ ,,
− 70·5 ,,	,,	,,	6½ ,,
− 71·0 ,,	,,	,,	7 ,,

The difference between the rays-zero and the metallic zero is thus found to be − 71 sc. divs., or − 0·5 of a volt. Immediately after this experiment, we removed the lead window and allowed the Röntgen light to shine on both metals, still 7 cms. apart. We then found the difference of zeros to be − 89 sc. divs., or − 0·64 of a volt; but instead of seven minutes, scarcely a quarter of a minute was taken to reach the rays-zero after the metallic connection was broken. These results are substantially in accordance with Erskine Murray's §§ 9 of his paper already referred to.]

Art. V.—Experiments on Electric Properties of Uranium.

(Read April 4, 1897.)

§ 25. Potential differences of uranium-conductance-zero from metallic zero for different metals in air.

We have used two different methods to measure the potential difference between two mutually insulated metals when the air between them is rendered conductive by the presence of uranium. The more convenient method is to take uranium as one of the mutually insulated metals. To do this we fixed a metallic disc, 3 cms. diameter, to the insulated terminal of a quadrant electrometer. Opposite this metallic disc, and separated from it by air, we placed a disc of uranium, 5·5 cms. diameter, connected to the other terminal of the electrometer. With this arrangement a steady reading, the metallic zero, was obtained when the quadrants of the electrometer were in metallic connection. After contact between the quadrants was broken at the electrometer a deviation from the metallic zero took place gradually to a point, the uranium-conductance-zero we shall call it, depending on the volta difference between the two opposed surfaces of metals, more or less tarnished as they generally are. On the other hand, if the insulated metal had a charge given to it of such an amount as to cause the electrometer reading to deviate from the metallic zero beyond the uranium-conductance-zero, the reading quickly fell to this conductance-zero and there remained steady. When no charge was given to the insulated metal the steady conductance-zero was reached in about half a minute. The following table gives the potential differences found in this way:—

Metals.	Potential Difference. Volts.
Polished aluminium (1) immediately after being polished,	−1·13
Polished aluminium (1), next day,	−1·90
Polished aluminium (2),	−1·00
Amalgamated zinc,	−0·80
Polished zinc,	−0·71
Unpolished zinc,	−0·55
Polished lead,	−0·54
Tinfoil,	−0·49
Unpolished aluminium (1),	−0·41
Polished copper,	−0·17
Unpolished copper,	+0·07
Silver coin,	+0·05
Carbon,	+0·20
Oxidised copper (*a*),	+0·42
Oxidised copper (*b*),	+0·90

With a third specimen of oxidised copper a potential difference

of $+0{\cdot}35$ of a volt was obtained. This specimen was afterwards connected to sheaths; a piece of polished aluminium was placed opposite it and connected to the insulated terminal of the electrometer. The uranium disc, insulated on paraffin, was then placed between them, and the deviation observed was equivalent to a potential difference of $-1{\cdot}53$ volts; that is, we obtained an effect equivalent to the sum of the effects we obtained when the metals were separately insulated in air opposite uranium.

§ 26. Instead of placing the uranium directly opposite the insulated metal in air we also observed the conductance-zero by mutually insulating two metals in air, one of which was transparent to the uranium influence.

For this purpose we made a tinfoil box, with tinfoil sufficiently thin to be transparent to the uranium influence. The tinfoil forming the box was connected to sheaths. Inside it another metal was insulated on a glass stem, and placed so as to be parallel to one end of the tinfoil box. This metal was connected to the insulated terminal of the electrometer. The uranium was placed outside the box, about half a centimetre distant from the end to which the insulated metal was parallel. The same conductance-zero was obtained with the uranium insulated, or with it connected to sheaths. The time required to reach the uranium-conductance-zero with this arrangement was usually four or five minutes, and charge given to the insulated metal large enough to produce a deviation beyond the conductance-zero was discharged till this zero was reached. A charge, causing the electrometer to deviate in the opposite direction, was discharged to the metallic zero and thence on to the uranium-conductance-zero, where it remained steady.

With polished aluminium as the insulated metal, the potential difference obtained was $-0{\cdot}7$ of a volt.

§ 27. Effect of various screens on the rate of reaching the zero.— With the second arrangement, described in § 25, it was possible to obtain a relative idea of the transparency to the uranium effect of screens of various materials. For example, when a sheet of lead, about 2 mms. in thickness, was placed between the uranium and the tinfoil, no deviation from the metallic zero was obtained. In other words, lead is not transparent to the uranium influence.

Glass 3 mms. thick did not entirely stop the deviation; it reduced the deviation in the first minute, however, to $\frac{1}{6}$ of the amount obtained with only air between the uranium and the outside wall of tinfoil. A copper screen, 0·24 mm. in thickness, reduced the rate to $\frac{1}{3}$; two copper screens, total thickness 0·48 mm., reduced it to $\frac{1}{12}$; three copper screens reduced it to $\frac{1}{40}$. A mica screen did not reduce the rate at all. A zinc screen, 0·235 mm. thick, reduced it to $\frac{1}{2}$. Two zinc screens, total thickness 0·47 mm., reduced it to $\frac{1}{7}$. Paraffin, 3 mms. thick, when placed between the two mutually insulated metals, stopped the deviation to the conductance-zero.

§ 28. Conductance-zero at different distances.

In the experiments described in the preceding section, the distances between the two mutually insulated metals was 2 cms. To observe the conductance-zero at different distances an aluminium box connected to sheaths was substituted in place of the tinfoil one, and oxidised copper insulated on a glass stem inside it. As before, with the tinfoil box, the uranium was placed outside the aluminium box, about 5 mms. from the end, to which the oxidised copper was kept parallel. The distance of the oxidised copper could be varied by moving the glass rod to which it was attached. The results obtained were as follows:—

Distance in cms.	Potential differences in volts.
1·5	+0·97
4·0	+0·98
0·5	+0·96
8·0	+1·03
2·0	+0·95

§ 29. Comparison of uranium-conductance-zero with water-arc-zero.

In the first arrangement for measuring the conductance-zero in § 25 we had discs of uranium and aluminium separated by air. We varied this by placing the uranium so close to the insulated aluminium that it could be brought into electric connection with it by a drop of water. The deviation obtained in the electrometer by this means was always in the same direction as the uranium-conductance-zero between the surfaces when dry, and was usually smaller in magnitude. For instance, with aluminium-air-uranium

the deviation was + 0·8 of a volt, with aluminium-water-uranium it was + 0·43 of a volt.*

§ 30. Uranium-conductance-zero in different gases and at different atmospheric pressures.

To investigate the behaviour of uranium in different gases and at different atmospheric pressures another piece of uranium 3 cms. long, 1 cm. broad, and $\frac{1}{2}$ cm. thick, was mounted firmly in a glass bulb 6 cms. long, 3 cms. diameter on a platinum electrode fused into one end of the bulb. The uranium in the glass bulb was surrounded throughout two-thirds of its length by a zinc cylinder $1\frac{1}{2}$ cm. in diameter. This zinc cylinder was kept in position by a stiff platinum electrode fused into the other end of the glass (see diagram 7). Two glass tubes were fixed on to the bulb, one at

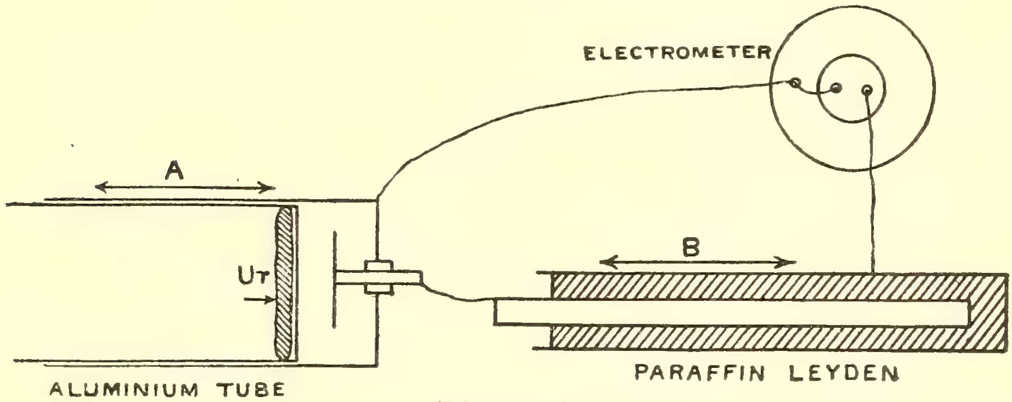


Diagram 7.

each end; by means of these any desired gas could be introduced, or any desired vacuum could be obtained.

The gas used was first stored in a reservoir over water. It was then bubbled through strong sulphuric acid and drawn over caustic potash, calcium chloride, and phosphoric anhydride into the glass bulb. The bulb was first exhausted to an atmospheric pressure of about 6 mms.; then the gas to be used was passed into it. This was repeated about twenty times. Finally it was strongly heated so as to drive off any adhering layers of gas, and then allowed to cool in an atmosphere of the gas at 760 mms.

* On the other hand, when the uranium surface was covered with water to the depth of about a millimetre, and an air space left between the wet uranium surface and the opposed insulated metal, so that we had a uranium-water-air-metal arc, the rate of deviation from the metallic zero was reduced so much as to be scarcely appreciable.

pressure. One of the tubes was then sealed up; the other was closed by a good fitting and well-greased glass stopcock.

The vacuums up to 2 mms. pressure were obtained by means of a double-barrelled air-pump. Higher vacuums were obtained by means of a Töpler pump.

To observe the conductance-zero the uranium was connected to the insulated terminal of the electrometer, and the zinc cylinder to sheaths. In the following table, the results obtained in air, hydrogen, and oxygen are given:—

	Difference of potential between the uranium-conductance-zero and metallic zero.		
Pressure in mms.	Hydrogen.	Oxygen.	Air.
760	+ ·17 of a volt. (in about a min.)	+ ·105 of a volt. (in about a min.)	+ ·11 of a volt. (in about a min.)
193	+ ·12 of a volt. (in about a min.)		
66	+ ·05 of a volt. (6 min.)	+ ·11 of a volt. (3 min.)	
8	+ ·04 of a volt. (8 min.)		
2	. . .	+ ·10 of a volt in 27 min.	
$<\frac{1}{1000}$	+ ·05 of a volt in 28 min.		

The uranium-conductance-zero between mutually insulated uranium and zinc differs much less from the metallic zero than in our previous experiments. This is probably due to the oxidation of the zinc of the zinc cylinder. The conductance-zero, however, it will be noticed, is approximately the same in all three gases.

§ 31. Leakage in air at ordinary pressure at different voltages.

We used in our first experiments the two Leydens method described in § 13. The Leyden B, whose external coating was connected to the insulated terminal of the electrometer, and its internal coating to the internal coating of A, was the paraffin Leyden described in § 13. The Leyden A was a cylinder of aluminium, with one end closed with aluminium. This formed the external coating. The internal coating was a disc of aluminium insulated in paraffin. The uranium was placed inside a cardboard cylinder, with one end open and the other covered with aluminium so as to touch the aluminium (see diagram 8).

This cardboard cylinder could be moved backwards and forwards in the aluminium cylinder, so that the distance between the insulated disc in the latter and the aluminium end of the former could be varied. The uranium influence thus acted through the aluminium end of the cardboard box, and made the air between the end and the insulated aluminium disc conductive. The leakage was in this way made slow enough to be easily observed on the electrometer. The rate of leak was not perceptibly increased when the piece of uranium was heated or when the sunlight fell on it. The aluminium end of the cardboard box and the outside coating of the aluminium cylinder were connected to sheaths.

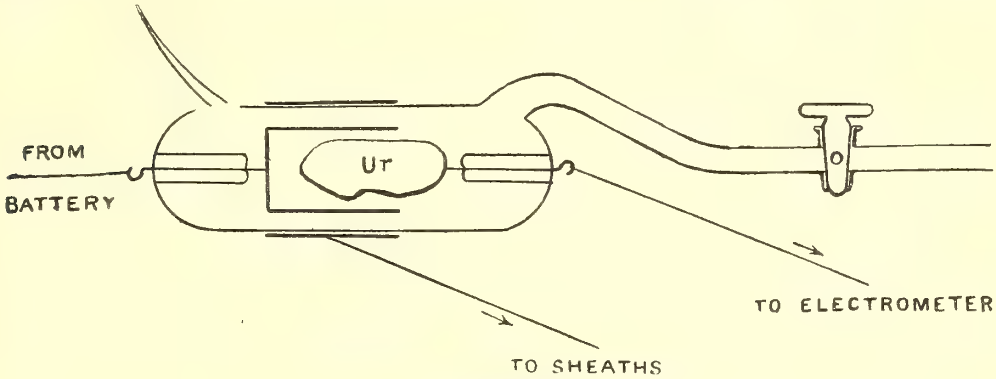


Diagram 8.

The insulated aluminium disc was connected to the inside coating of B. The inside coatings were charged to a known potential, and then left to themselves.

The air space between the insulated aluminium disc and the aluminium end of the cardboard box was 2 centimetres. The voltages used were therefore voltages per two centimetres of air space. With this arrangement the leakage per minute at different voltages was :—

Voltage.	Leakage per minute in scale divisions.
6	56
10	65·5
44	113
88	128
176	156
750	219
1250	229
2000	260
3000	276

[Sensibility of electrometer 24 sc. divs. per volt of subsidence of difference of potential between coatings of A.]

We also measured the leakage at different voltages with the zinc cylinder in the glass bulb described in § 30, charged to a definite potential, and the uranium connected to the insulated terminal of the electrometer. The voltages up to 90 volts were obtained by connecting one terminal of a battery to the zinc, and keeping it connected during the experiments, while the other terminal was connected to sheaths. For higher voltages the zinc was charged to the given potential, and then disconnected from the charging body.

Voltage per 2 mms.	Leakage per minute in scale divisions.
2	92
4	100
22	120
92	129
132	138
200	130
300	137
415	136

[Sensibility of electrometer 140 sc. div. per min.]

The appended curves (diagram 9) were drawn by taking the leakage per minute as ordinate, the voltage as abscissa. Curve *a*

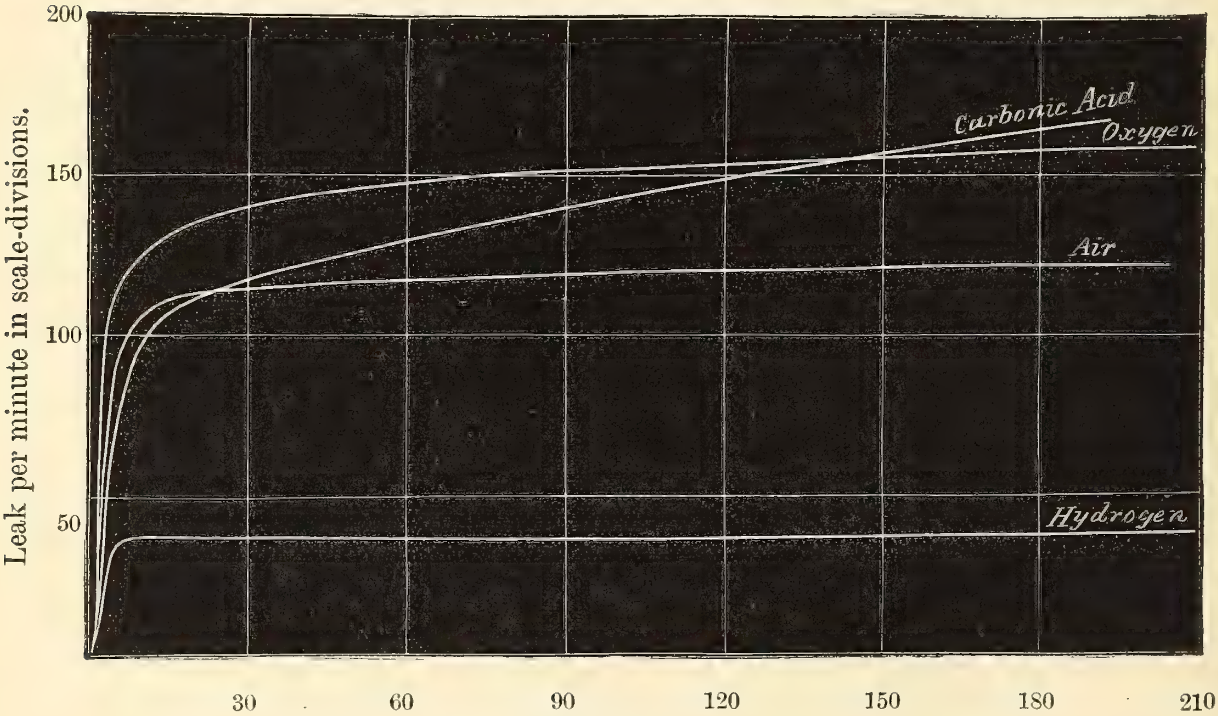


Diagram 9.

gives the results obtained with the two Leydens arrangement reduced to voltages per 2 mms. between the internal and external

coatings of A. Curve *b* gives the results obtained with the smaller piece of uranium in the glass bulb.

§ 32. Leakage in other gases at ordinary pressure.

We have also observed the rate of leaks in hydrogen, oxygen, and carbonic acid at ordinary pressure at different voltages. The glass bulb referred to in §§ 30 and 31 was used for this purpose. The voltages were obtained by connecting the zinc to one terminal of a battery, and the other terminal to sheaths. The uranium was connected to the insulated terminal of the electrometer. While the connection between the battery and the zinc was being made the uranium was put in metallic connection with the case of the electrometer; afterwards it was disinsulated, and the deviation in the electrometer observed per minute for a number of minutes. The following results were obtained for these gases:—

Voltage per 2 mms.	Leakage per minute in scale divisions.
HYDROGEN.	
2	32
4	37
22	39
34	38
100	39
135	38
OXYGEN.	
4	125
96	157
CARBONIC ACID.	
4	94
95	167
238	183
255	180
2900	Discharge by sparking.

[Sensibility of electrometer 140 sc. div. per volt.]

The results given for these three gases are comparable to the second series of results given in § 7 for air.

We see that the rate of leak is greater in oxygen than in air; no comparative figures need be given, as these would vary according to the voltage chosen.

The leakage in hydrogen is less than that in air; in carbonic acid it is less for 4 volts, but greater for 90 volts, than it is in air; for the latter voltage the leakage in carbonic acid is greater even

than the corresponding leakage for oxygen. The appended curves show the peculiarities of the leakage in the different gases (diagram 10).

§ 33. Leakage in different gases at different atmospheric pressures.

The method of filling the glass bulb with any given gas, and the way in which the different vacuums were obtained, has been described in § 30.

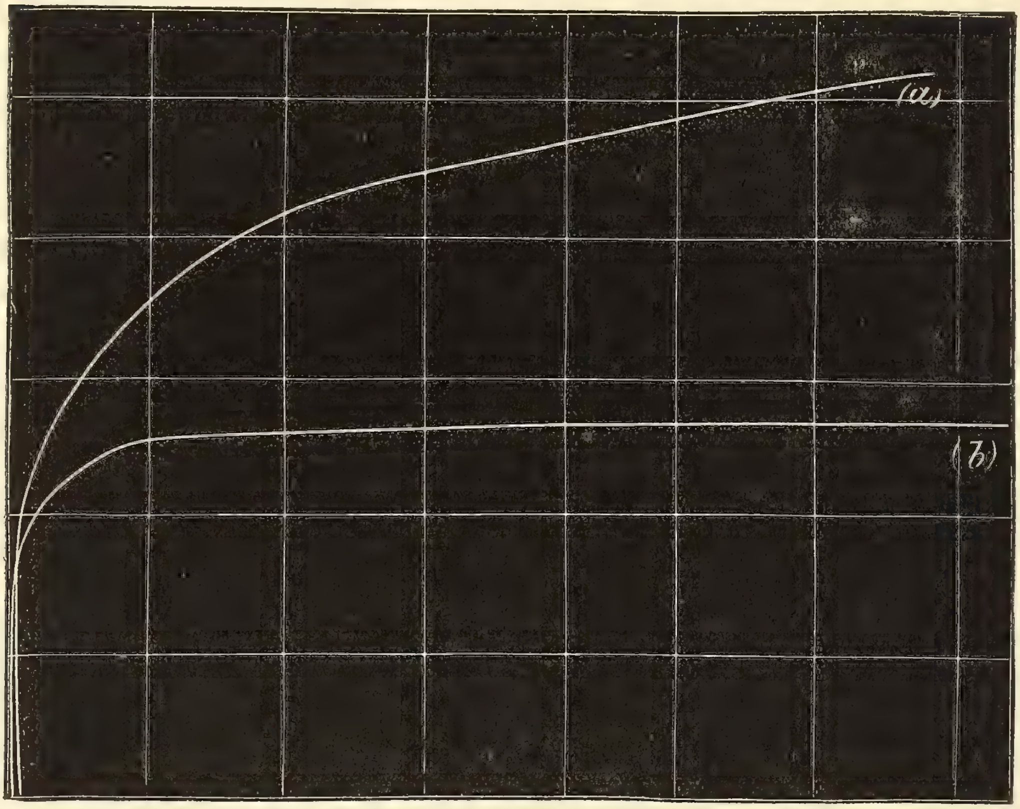


Diagram 10.

The following tables give the results obtained with the gases we have up till now experimented on.

Air.

α. Atmospheric Pressure in mms.	β. Leakage per min. for 4 Volts.	γ. Leakage for 96 Volts.	$\frac{\beta}{\alpha}$.	$\frac{\gamma}{\alpha}$.
760	100	131	·132	·172
240	44	46	·183	·192
190	40	39	·210	·205
121	24	26	·197	·214
64	12	13·5	·187	·212
58	11	10·0	·189	·172
23	4·4	3·75	·191	·163
3·6	1·2	1·2	·339	·339

It will be seen from the last two columns of the table that the rate of leak at 4 volts and at 96 volts is nearly proportional to the atmospheric pressure. The results obtained at 3·6 mms. are not very reliable. With lower pressures no appreciable leakage at these two voltages was observed.

Hydrogen.

α. Atmospheric Pressure.	β. Leakage per Minute at 4 Volts per 2 mms.	$\frac{\beta}{\alpha}$.	$\frac{\beta}{\sqrt{\alpha}}$.
760	37	·0487	1·34
197	11	·056	·77
66	4	·061	·49
8	1·5	·187	·53

With lower pressures no leakage was observed. The leakage is at higher pressures somewhat approximately proportional to the pressure, at lower ones to the square root of the pressure.

Oxygen.

α. Atmospheric Pressure in mms.	β. Leakage per Minute for 4 Volts per 2 mms.	$\frac{\beta}{\alpha}$.	$\frac{\beta}{\sqrt{\alpha}}$.
760	125	·16	4·5
205	48·5	·236	3·38
64	15·0	·234	1·87
2	2·0	1·0	1·414

Carbonic Acid.

Atmospheric Pressure in mms.	Leakage per Minute for 4 Volts per 2 mms.	Leakage per Minute for 100 Volts per 2 mms.
760	94	167
62	18	21
2	...	Not observable.

The curves for air, oxygen, and hydrogen given in diagram 11 were obtained by taking the atmospheric pressure in mms. as abscissa, and the leakage per minute for 4 volts as ordinate.

§ 34. Voltage necessary to produce spark at different atmospheric pressures.

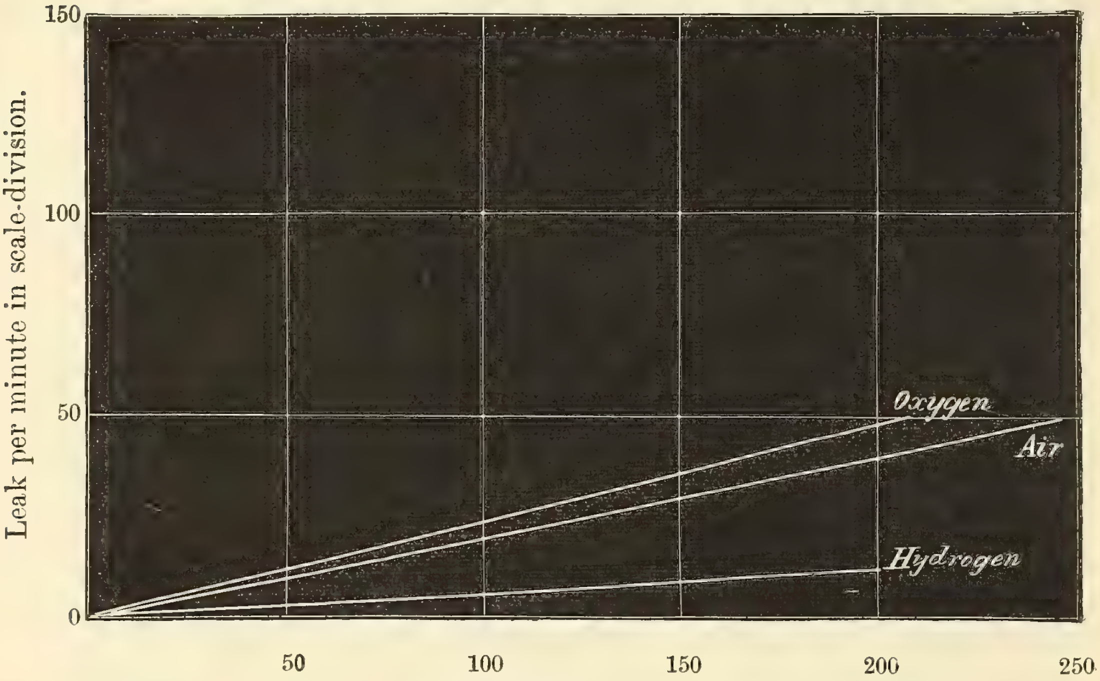


Diagram 11.

We found that, at ordinary atmospheric pressure, sparking took place in air at 4800 volts. At 232 mms. pressure the potential necessary to produce a spark fell to between 1500 and 2000 volts. At 127 mms. it had fallen to between 1100 and 1300 volts. At 54 mms. it was 700 volts; at 7 mms. 420 volts; at 2 mms. about 400 volts. At about $\frac{1}{1000}$ mm. the voltage had risen again to 2000 volts.

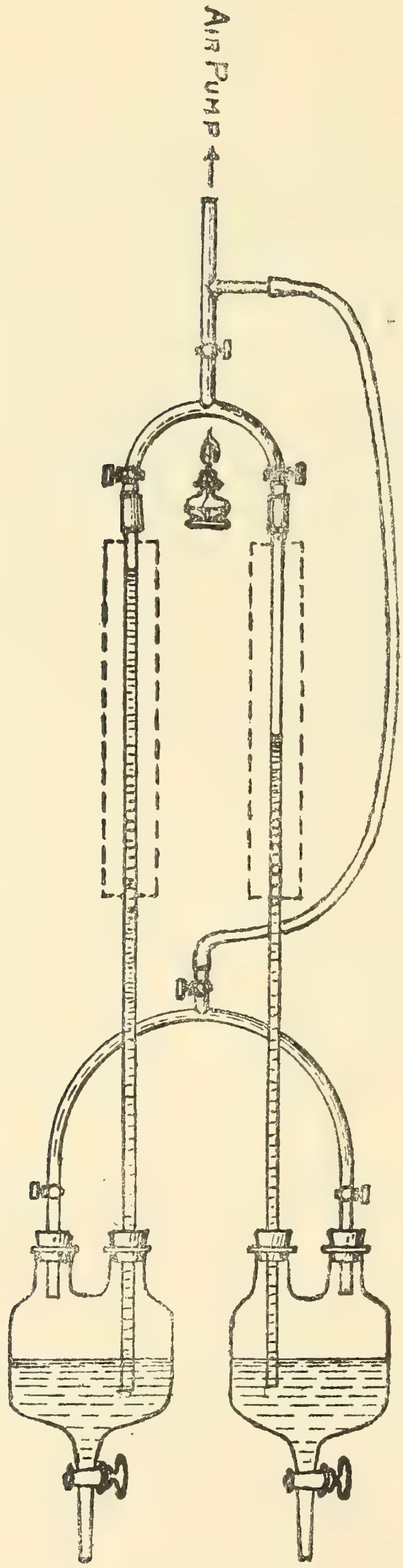
On a Differential Method for Measuring Differences of Vapour Pressures of Liquids at one Temperature and at Different Temperatures. By Lord Kelvin, G.C.V.O.

(Read January 18, 1897.)

§ 1. Apparatus for realising the proposed method is represented in the accompanying diagram. Two Woulffe's bottles, each having a vertical glass tube fitted air-tight into one of its necks, contain the liquids the difference of whose vapour pressures is to be measured. Second necks of the two bottles are connected by a bent metal (or glass) pipe, with a vertical branch provided with three (metal or glass) stopcocks, as indicated in the diagram. Each bottle has a third neck, projecting downwards through its bottom, stopped by a glass stopcock which can be opened for the purpose of introducing or withdrawing liquid. The upper ends of the glass tubes are also connected (by short india-rubber junctions or otherwise) with a bent metal pipe carrying a vertical branch for connection with a Toepler * mercury air-pump. This vertical branch is provided with a metal stopcock. The vertical branch of the pipe fitted into necks of the two bottles is also connected to the air-pump as indicated in the drawings.

§ 2. To introduce the liquids, bring open vessels containing them into such positions below the bottles that the necks project downwards into them. Close the glass stopcocks of these lower necks, open all the other six stopcocks, and produce a slight exhaustion by a few strokes of the air-pump. Then, opening the glass stopcocks very slightly, allow the desired quantities of the liquids to enter, and close them again. They will not be opened again unless there is occasion to remove the whole or some part of the

* An ordinary mechanical air-pump would not serve the purpose, because its valves would not open properly to draw out the very small amount of air which must be removed to avoid vitiating the observations by any sensible amount of air-pressure added to the pressure of the vapour.


AIR PUMP

liquid from either bottle; and, unless explicitly mentioned, will not be included among the stopcocks referred to in what follows. It will generally be convenient to make the quantities of the two liquids introduced such that they stand at as nearly as may be the same levels in the two bottles, as indicated in the drawing.

§ 3. Operate now on one only of the liquids until it is got into equilibrium, with its upper level at some point in its glass tube, and nothing but its own vapour between this surface and the closed stopcock immediately above it. To do this proceed as follows:—Close and keep closed the two stopcocks of the liquid not operated on, and work the air-pump with the other four stopcocks all open until an exhaustion, not quite as perfect as is possible, of the air over the liquid operated on is produced.

§ 4. Then close the lower air-pump stopcock, and go on working the pump until the liquid in the tube ceases to rise further above its level in the bottle. Close the two stopcocks of this liquid.

§ 5. Operate similarly on the other liquid.

§ 6. Close now the lower air-pump stopcock, and equalise the pressures of air and vapour above the liquids in the two bottles by opening their neck stopcocks. If the levels of the liquids in the two columns are lower than convenient for observation, some air should be allowed very cautiously to run back from the air-pump into the two bottles through the lower air-pump stopcock. After doing this, repeat the operation of § 4 for each liquid.

§ 7. The operation of § § 4, 5 must be continued long enough to distil out of the upper part of each liquid, in its glass tube, air or any foreign * volatile substance sufficiently to prevent any sensible pressure on the free surface other than that of the vapour of the solvent.

§ 8. By proper thermal appliances, indicated by the dotted lines in the diagram, and the lamp under the upper bent metal tube (inserted merely as an indication that somehow the metal tube is to be always slightly warmer than the warmer of the two liquid surfaces, in order that there may be no condensation of vapour in it), bring the upper surfaces of the liquids to any one temperature, or to two different temperatures. The difference of levels of the

* Compare Ostwald, *Physico-Chemical Measurements*, translated by Walker (Macmillan, 1892), last paragraph, page 112.

liquids in the two tubes, with proper correction for the densities of the two liquids at their actual temperatures in different parts of their columns, gives the difference of vapour pressures for the actual temperatures of the two liquids at their upper surfaces.

§ 9. To facilitate and approximately determine the hydrostatic correction for specific gravities at the actual temperatures of the two liquids, open wide the stopcock above the top of one of the two glass tubes, and let a little air run back from the air-pump, by very cautiously and slightly opening our upper air-pump stopcock, and closing it again before the liquid surface reaches the lower end of its glass tube. Then open wide the stopcock over the top of the other glass tube. After that, by cautiously opening and closing our lower air-pump stopcock, let in a little air to the bottles until the mean level of the liquids in the two columns rises to nearly the same level as it had in the observed positions of § 8. In the present circumstances, air in the upper bent metal tube resists diffusion of vapour through it sufficiently to prevent any important difference of temperatures from being produced by evaporation and condensation at the two liquid surfaces, and there is practically perfect hydrostatic equilibrium of equal liquid pressures at the tops of the two columns.

§ 10. The vapour pressure of water is accurately known through a very wide range of temperature from Regnault's experiments; hence, if pure water be taken for one of our two liquids, the mode of experiment described above determines the vapour pressure of the other liquid.

§ 11. The apparatus may be kept day after day with the same liquids in it (all the stopcocks to be closed, except when it is not in use for observations); and thus the observations for difference of vapour pressures may be repeated day after day; or a long series of observations may very easily be made to determine vapour pressures at different temperatures. Always before commencing observations the operation of § 7 must be repeated to remove air or other volatile impurity, if any has escaped from dissolution in either liquid into the vapour space above it, or if any air has leaked in by the upper stopcocks.

On Ethane prepared from Ethyl Iodide, and on the Properties of some Mixtures of Ethane and Butane. By Professor J. P. Kuenen. (With Three Plates.)

(Read April 5, 1897.)

In a paper on mixtures of ethane and nitrous oxide, read before the Physical Society of London in May 1895,* I communicated a set of observations with regard to the condensation and the critical state of ethane. This substance was prepared by electrolysing sodium acetate: it was purified with fuming sulphuric acid, caustic soda, and phosphorus pentoxide, and condensed in a small copper cylinder, where it was subsequently boiled at low temperature in order to expel all permanent gas. The ethane with which the glass compression-tubes were filled was drawn from the liquid contained in the copper cylinder. Its condensation-pressures and critical constants are contained in the following table, which is taken from the paper mentioned (comp. fig. 3):—

Table I.—*Ethane.*

t.	*p.*
5·85	27·4
10·65	30·45
15·4	33·8
22·4	39·7
29·35	45·9
31·0	47·6
32·0	48·8 Critical point.

The values of p and of the critical constants in Table I. are not absolutely correct. The observations showed the substance to contain some impurity, the condensation-pressures not being quite constant, but showing a slight increase, as is always the case for mixtures. The difference of the pressure at the beginning and at the end of the condensation amounted to 0·43 atmospheres at 15° C. It is, however, unlikely that this impurity in the ethane can have affected the values for p by more than a few tenths of

* *Phil. Mag.*, 40, pp. 173–194.

an atmosphere. Nor will the value for the critical temperature differ by more than 0·1 or 0·2° C. from the true value, considering that Andrews' carbonic acid showed changes in vapour-pressure of over two atmospheres under the same circumstances, and that his value for the critical temperature, 30·9° C., is only about 0·4° C. too low.

The values obtained by others for the vapour-pressures, and especially for the critical constants, differ materially from mine. Dewar* gives 35° C. for the critical temperature, Olszewski† 34° C. and 50·2 atmospheres for the critical pressure, Haenlen‡ from 32°–40°, probably 34·5° C. and 50 atmospheres. The vapour-pressures obtained by Olszewski and Haenlen are given in Table II.

Table II.—*Ethane.*

1. Olszewski.			2. Haenlen.		
t.	*p.*		*t.*	*p.*	
−93	1		−31	11	
+0	23·8		−20	14·5	
+23·5	40·4		−11	18·3	
+29	46·7		+0	23·3	
+34	50·2	Critical point.	+15	32·3	
			+34·5	50	Critical point.

Haenlen's values are constantly a little lower than mine; Olszewski's, at higher temperatures, a little higher. Olszewski prepared his ethane from $(C_2H_5)_2Zn$, Haenlen from ethyl iodide by the method of Gladstone and Tribe, as applied by Frankland.§

As I was desirous to ascertain the reason for the discrepancy between the different results, I prepared some ethane by Frankland's method, and the results are contained in this paper. The gas passed through a "scrubber," fuming sulphuric acid, bromine and caustic potash, was collected in a gasometer, dried with lime and phosphorus-pentoxide, and compressed|| into a copper cylinder similar to the one used by me on former occasions.

* *Phil. Mag.* (5), 18, p. 214.

† *Bulletin Ac. des Sciences de Cracovie*, 1889, p. 27.

‡ *Liebig's Annalen*, 282, p. 245.

§ *Jour. Chem. Soc.*, 45, p. 154 ; 47, p. 236.

|| The compression-pump was a Natterer, which was very kindly lent to me by Prof. Crum-Brown.

This copper cylinder is provided with two outlets, one at the top and the other starting near the bottom. Ethane may, therefore, either be taken from the gas portion, or from the liquid portion of the condensed substance. The first sample was taken from the liquid, and will be called mixture *a*. The results obtained with this sample are contained in Tables III.–VI., XII., figs. 1 and 3.

The apparatus employed was Ducretet's apparatus, a tube with air being used as a manometer. The full description of apparatus and method of calculation will be found in former papers.* The only point to be noticed is the application of a little soft-iron stirrer inside the compression-tube, which is moved up and down by an electro-magnet outside the water-jacket which keeps the temperature constant. The pressures are measured in atmospheres, the volumes in an arbitrary unit, but in such a manner, that the volumes belong to equal original volumes (at 0° C. and 1 atmosphere) for both mixtures.

The substance *a* behaved like a mixture, as regards both condensation and critical phenomena. The critical region, an expression which will be explained later on, ranged from 35·25 to 35·7.

A second sample was taken from the gas portion in the copper cylinder. This sample (mixture *c*) appeared to be accidentally almost identical with mixture *a*, the critical temperature being 35·75, and the condensation-pressures at the beginning and at the end being only a little lower than for mixture *a*, e.g., $p_e = 32{\cdot}78$ at 14·95, instead of 32·91. No further observations were made with this mixture.

A third sample was again taken from the liquid. The results for this sample (mixture *b*) are found in Tables VII.–XI., XIII., and figures 2 and 3.

The critical region for this gas lies between 38·35 and 39·25° C.

The possibility of drawing from the cylinder samples of ethane differing so widely from each other in physical properties confirms the presence of a mixture in the cylinder.

In order to get some idea as to the nature of the admixture,

* *Archives Néerlandaises*, 26, pp. 354–422; Communications from the Physical Laboratory, Leiden, Nos. 4, 7, 8, 11, 13, 16, 17. *Zeitschrift Physik. Chemie*, 11, pp. 38–48.

different samples of the gas were weighed. The volume of the glass bulbs used for this purpose was about 130 c.c. Mixture *a* could not be weighed, as no sufficient quantity had been kept for the purpose. Mixture *b* and *c*, as well as a fourth sample (*d*), were weighed and compared with air. They gave the following results :—

Air,	28·88		
Mixture *c* (= *a*),	30·73	. . cr. temp. . . .	35·75
Mixture *b*,	31·37	. . ,, . . .	39·25
Mixture *d*,	32·52		

The normal weight for ethane would have been 30. If we extrapolate the critical temperature of pure ethane from the values for *c* (practically the same as *a*) and *b*, we find 31·8° C., which is in good agreement with the value obtained for almost pure ethane prepared from sodium acetate.

The substance mixed with ethane, as prepared by this method, is evidently heavier than ethane. Moreover, it lowers the condensation pressures and raises the critical temperature (fig. 3). All this is compatible with the supposition that the admixture is butane — a very probable supposition, when we consider the possible chemical reactions taking place in the preparation. The critical constants of butane are unknown, but the critical temperature must be somewhere about 150° C., the point for propane being 100° C. (Olszewski, Haenlen), and for pentane about 197° C. (S. Young). The same substance seems to have occasioned Haenlen's abnormal results (a higher critical temperature and lower pressures), and possibly the same is true for Dewar's ethane.

Supposing that the impurity is butane, the weights of the mixtures show that *a* and *b* contained 2·5 and 5 per cent. butane respectively, or 4·7 and 9·2 per cent. when expressed in parts by weight, quantities the presence of which could not be very easily detected by gas-analysis.

It is not impossible that by discarding the scrubber where the metal is apt to act upon the iodide if too little alcohol is present, a purer product may be obtained, but now that the formation of butane is once proved, and as it can hardly be entirely prevented, the method loses much of its importance for the preparation of a pure gas. The separation of two hydrocarbons like

ethane and butane is an extremely difficult matter. I know of no method except fractional distillation. The quantity left after all my experiments were finished was too small for the purpose.

We will now consider the diagrams (figs. 1–3) more closely, assuming the mixtures to contain ethane and butane.

The reason why it seemed worth while to communicate the numerical results and the curves which are based upon them is, that the mixtures form a type which has not been investigated before. Andrews' curves for carbonic acid really belong to a mixture of CO_2 and a trace of permanent gas, say air, *i.e.*, of a substance of much lower critical temperature than carbonic acid. The features of his isothermals inside the border curve are well known. They start almost horizontally from the point where the condensation begins (*b.c.*), but are really slightly curved, especially near the end (*e.c.*), the curves turning their concave side upwards.

The mixtures of ethane and butane, on the other hand, are mixtures of one substance with a small quantity of a second substance of much higher critical temperature and lower vapour-pressures. The isothermals inside the border-curves have a correspondingly different shape. The curves turn their concave side downwards, the curvature being relatively high near the beginning, and diminishing towards the end. If air had been present in the mixtures as well as butane, it would have shown itself in a change in curvature in the curves near *e*, as in Andrews' curves.

The values for *p* and *v* at the end of the condensation may be read with great accuracy, as the volume is small and consequently stirring is very effective: concordant values for those points are easily obtained. This is not the case as regards the points *b.c.*, especially not at low temperatures: the column occupied by the substance is very large in those cases, and it is difficult to obtain perfect homogeneity of the mixture; even then, however, the point where condensation commences is not easily determined (at least not at low temperatures) owing to the difficulty of observing the minute quantity of liquid which is formed in the beginning. Some of the *b.c.* points (in the tables XII. and XIII. printed in italics) have therefore been obtained by interpolation as the point of intersection of the two parts of the isothermals inside and outside the border-curve. This intersection always takes place at a

certain angle. Point *b.c.* for 25·35° C. (mixture *a*, fig. 1), as determined without special accuracy, will be seen to fall entirely outside the real border-curve, as determined by interpolation.

The explanation of the shape of the isothermals inside the border-curve is simple: the liquid which is formed near *b.c.* is butane with some ethane dissolved in it. As the quantity of butane is relatively small, the condensation of butane changes the composition of the mixture rapidly, and, accordingly, the condensation pressure also. But the further the process is continued the smaller the changes in composition, and therefore also in the pressures, become. With carbonic acid and a little air it is just the reverse: the liquid condensing in the beginning is CO_2 with a trace of air: but the composition of the mixture above the liquid hardly changes at all in the beginning: it is only in the end that the mixture becomes appreciably richer in air, and shows a slight rise in pressure.

Andrews' mixture contained so little air that the critical phenomena, even when tested with a view to detecting an influence of the admixture, would probably not have shown any abnormalities. The mixtures of ethane and butane, *a*, and in a greater degree *b*, contained sufficient quantities to allow the critical phenomena typical of mixtures to be completely followed.

The peculiar features of the *pv* diagram for mixtures near the critical point were described by me in an article in the April number of *Science Progress*, 1897.

The isothermal for the critical temperature (as for pure substances) touches the border-curve, but for mixtures not at the top M, but at a point to the right of M, C in the diagrams 1 and 2. For the critical isothermal $\frac{dp}{dv} < 0$ in all its points. For the isothermals at low temperature, a theoretical part with the double waveshape, as proposed by James Thomson and Van der Waals, must be assumed, and the transition from these isothermals to those without an unstable part takes place *inside* the border-curve for an isothermal of lower temperature than *t*C. That temperature is the critical temperature of the mixture as it would be, if it remained homogeneous and did not separate into phases of different composition.

The peculiarities of the condensation at temperatures below the critical temperature do not come out properly in a *pv* diagram. These phenomena, as deduced from Van der Waals' theory of mixtures, are found in previous papers already quoted.

Below *t*C (39·25 for *b*, 35·7 for *a*) the condensation is retrograde, that is to say, liquid is formed by compression, the quantity of which first increases and then diminishes and disappears. Just below the critical temperature the quantity of liquid formed is only very small, but the lower the temperature the larger the maximum quantity of the liquid becomes. At last temperatures are reached where the "critical phenomenon" takes place, first at the bottom of the column and then at higher levels as the temperature falls. The critical phenomenon is here meant to comprehend all the phenomena which are displayed when the coexisting phases approach each other and finally coincide: they consist chiefly in the disappearance of the liquid surface and the formation of a characteristic bright blue mist in the substance. The temperature at which the critical phenomenon occurs about at the middle of the tube is the plait-point temperature *t*P (38·35 for *b*, 35·25 for *a*). A few tenths of a degree lower the condensation is normal, the quantity of the liquid increasing during compression until the whole of the substance is liquid. The critical phenomenon would be confined to the plait-point temperature, if it were not for gravitation. Gravitation makes the density and composition of the mixture different at different levels, and the consequence is a small range of temperatures (38·2–38·7 for *b*), at which the critical phenomenon takes place at different levels, the higher the temperature the lower the level.

The point indicating the end of the condensation at the plait-point temperature corresponds to the plait-point in Van der Waals' theory. This point (P in the diagram) does not coincide with the top of the border curve M, as will be seen even better in the tables, especially for mixture *b*. Evidently P is to the left of M, C being to the right of M.

In the *pt* diagram (fig. 3) are given the vapour-pressure curve for ethane and the two looped vapour-pressure curves for the mixtures. The curve which envelops the loops is the plait-point curve, the point of contact P corresponding to the plait-

points on Van der Waals' thermodynamical surface, and to the points P in figs. 1 and 2. The tops of the loops M correspond to the tops of the border-curves M in the *pv* figures. C is the point where a loop has a tangent parallel to the *p* axis. Above the temperature belonging to that point no condensation takes place, but the critical phenomenon belongs to the temperature of the plait-point which is always lower. The circumstance that the plait-point curve rises agrees with the fact that P is found to be to the left of M.

It is impossible to say anything about the exact course the plait-point curve will follow outside the field of observation, except that the critical temperature of the second substance seems to be higher than for ethane, and the vapour-pressures lower. The diagram, as far as we can judge, agrees entirely with the supposition of butane being the second substance, but this point can only be decided by preparing butane with a view to determine its constants, and by mixing it with pure ethane as prepared by electrolysis. If there was a simple law connecting the critical constants of mixtures with those of the constituents, we might calculate the constants for the second substance. But such is not the case. Pawlewski's law that the critical temperature is proportional to the composition expressed in weight units is very inaccurate, the deviations being sometimes considerable in both directions.

TABLE III.—*Mixture a.* (Fig. 1.)*

t.	*p.*	*v.*		$v_{liq.}$
14·95	25·73	268·5		...
14·95	26·90	249·7		...
14·95	28·26	230·3		...
14·95	29·27	215·2	*l*	–
14·95	29·45	210·7	*l*	–
14·95	29·66	207·3	*l*	–
14·95	29·78	204·4	*l*	–
14·95	30·32	190·	*l*	–
14·95	30·95	172·2	*l*	4·4
14·95	30·96	170·8	*l*	4·4
14·95	31·76	135·2	*l*	10·4
14·95	32·32	91·3	*l*	19·7
14·95	32·57	66·1	*l*	25·8
14·95	32·73	48·4	*l*	29·9
14·95	32·91	34·3	*e.c.*	34·3
14·95	40	33·6		33·6
14·95	50	32·6		32·6

* In the tables *l* indicates that the substance is partly liquefied.

TABLE IV.—*Mixture a.* (Fig. 1.)

$t.$	$p.$	$v.$	$v_{liq.}$
24·75	27·56	267	...
24·85	30·51	226·7	...
24·8	34·31	184·2	...
24·75	36·01	165·8	...
24·85	38·03	143·1	*l* –
24·85	38·46	132·3	*l* –
24·8	39·03	118·4	*l* 7·1
24·85	39·36	105·6	*l* 10·9
24·85	39·82	87·8	*l* 16·8
24·85	40·29	62·2	*l* 26·7
24·85	40·62	38·0	*e.c.* 38·0

TABLE V.—*Mixture a.* (Fig. 1.)

30·35	44·65	74	*l* 19·7

TABLE VI.—*Mixture a.* (Fig. 1.)

35·65	49·65	67·1	*l* –

TABLE VII.—*Mixture b.* (Fig. 2.)

14·95	25·42	268	*l* ? –
14·95	26·44	245	*l* –
14·95	28·70	194	*l* –
14·95	29·53	170	*l* –
14·95	31·05	112·2	*l* –
14·95	31·97	60·2	*l* –
14·95	32·65	34	*e.c.* 34

TABLE VIII.—*Mixture b.* (Fig. 2.)

24·95	27·76	262·5	...
24·95	29·85	235	...
24·95	31·72	212	...
24·95	33·78	187·5	...
24·95	34·68	173·5	*l* –
24·95	35·64	158	*l* –
24·95	36·97	134·1	*l* –
24·95	38·33	102	*l* 14·6
24·95	39·09	79	*l* 21·5
24·95	39·91	50·8	*l* 31·3
24·95	40·22	37·7	*e.c.* 37·7

TABLE IX.—*Mixture b.* (Fig. 2.)

32·55	28·96	264·5	...
32·55	31·16	237	...
32·55	33·96	206·5	...
32·55	36·85	178·5	...
32·55	38·01	168	...
32·55	39·51	154·5	...
32·55	40·85	142·3	...

TABLE IX.—*continued.*

t.	*p.*	*v.*	$v_{liq.}$
32·55	41·64	133	*l* –
32·55	43·03	115	*l* –
32·55	44·31	95	*l* 10·5
32·55	45·26	57·7	*l* 19·9
32·55	46·40	48	*l* 37·5
32·55	46·58	42·8	*e.c.* 42·8

TABLE X.—*Mixture b.* (Fig. 2.)

37·95	42·09	148	...
37·95	43·39	137·7	...
37·95	45·02	125·1	...
37·9	46·32	114·7	...
37·95	47·52	104·2	...
37·95	48·62	91·5	*l* –
37·9	49·22	83	*l* –
37·95	50·23	70	*l* –
37·95	50·68	61·5	*l* –
37·95	51·02	54	*e.c.* 54

TABLE XI.—*Mixture b.* (Fig. 2.)

38·75	49·59	88·3	*l* –

TABLE XII.—*Mixture a.* (Figs. 1 and 3.)

t.	p_b.	p_e.		v_b.	v_e.	
5·35	...	26·70		...	32·6	
11·35	...	30·47		...	33·6	
14·95	*29·02*	32·91		*220*	34·3	
20·15	33·65	36·86		176·7	36·5	
24·85	*37·84*	40·62		*147*	38·0	
25·35	38·40	41·18		151	38·5	
30·35	43·15	45·35		117·5	42·4	
32·95	45·92	47·67		100·7	46·4	
34·95	48·18	49·49		87·4	55·8	
35·15	48·55	49·55		83·4	55·3	
35·35	48·83	49·65	P	80·9	57·7	P
35·45	...	49·72	M	...	62·2	M
35·55	...	49·69	M	...	62·2	M
35·65	49·57	49·71	M	71·1	66·1	M
35·7	49·65 C			68 C		

TABLE XIII.—*Mixture b.* (Figs. 2 and 3.)

14·95	...	32·65		...	34	
24·95	*33·8*	40·22		*187*	37·7	
32·55	*41·1*	46·58		*140*	42·8	
37·95	*48·0*	51·02		*100*	54	
38·15	...	51·09		...	58	
38·35	...	51·12	P	...	60	P
38·75	49·00	51·24	M	93·3	64	M
39·0	49·93	51·23	M	86	68	M
39·15	50·29	51·15		84	70·5	
39·25	50·93 C			76 C		

KUENEN. PROPERTIES OF ETHANE AND BUTANE.

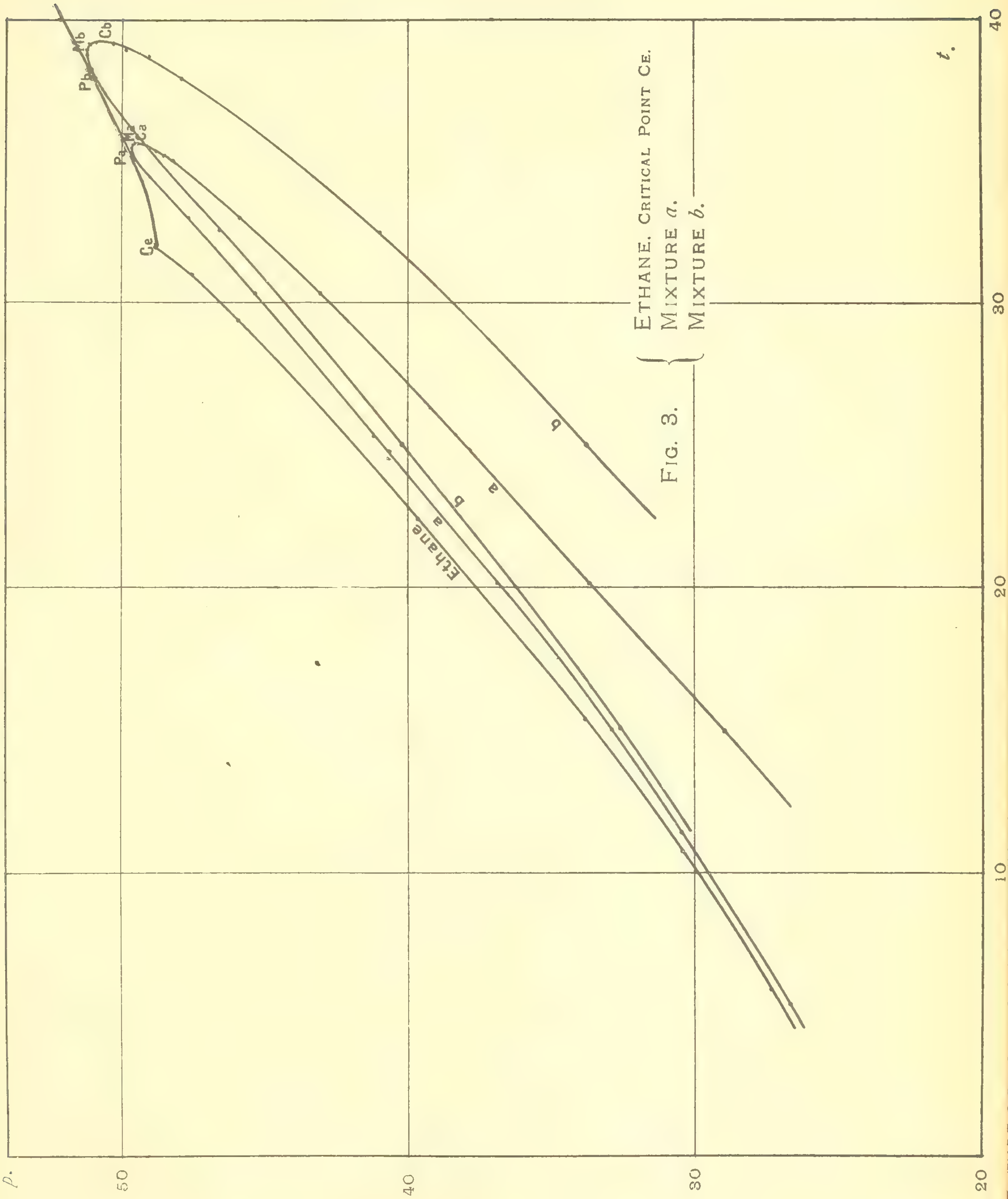


FIG. 3.
{ ETHANE. CRITICAL POINT Ce.
MIXTURE *a*.
MIXTURE *b*.

A. RITCHIE & SON, EDI

KUENEN. PROPERTIES OF ETHANE AND BUTANE.

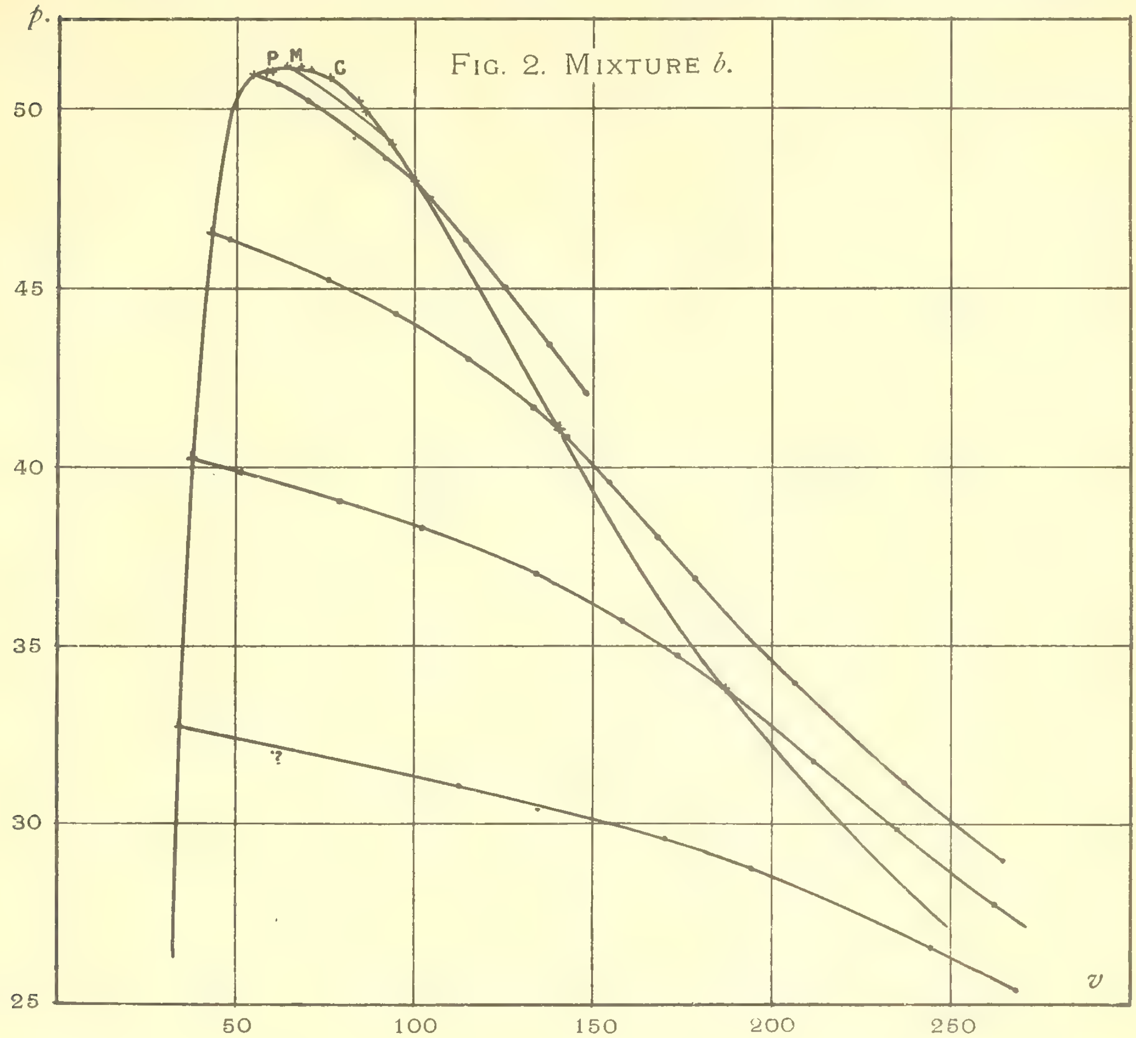


FIG. 2. MIXTURE *b*.

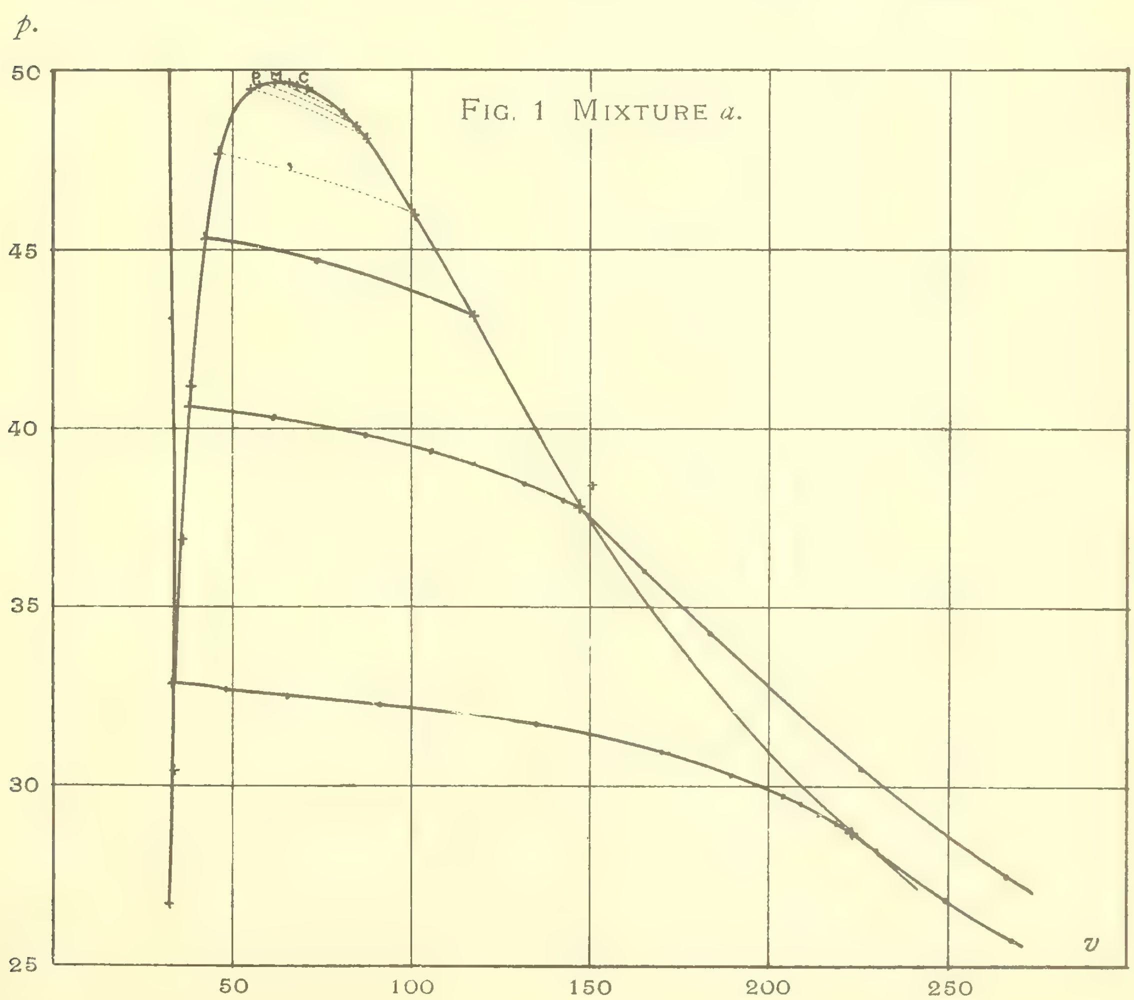


FIG. 1 MIXTURE *a*.

Our Perception of the Direction of Sound. By Albert Alexander Gray, M.D. (Glasgow). *Communicated by* Professor M'Kendrick.

(Read April 5, 1897.)

Our power of estimating the direction of sound is much less accurate than that of estimating the direction of light; and, further, our knowledge as to the way in which this perception of direction occurs is much more limited in the case of sound. We judge with the greatest accuracy the direction of a ray of light, and we know that we do so almost entirely by the aid of the muscular sense. How do we judge the direction of a sound?

Before attempting a reply to this question it will clear that matter up, to a certain extent, by giving a statement of a few facts which have been found out by experiment, and are well attested by various writers:—

(1) Individuals who are deaf of one ear have little power of estimating the direction of sound.

(2) Individuals who are blind of both eyes generally show a power of judging the direction of sound far superior to those gifted with sight. This is particularly true if the eye affection dates from childhood.

(3) The power of estimating the direction of sound depends upon its pitch and its quality. A pure tone of low pitch is very difficult to localise; with the higher notes this becomes easier, and with a compound sound it is also easy.

(4) The accuracy with which we estimate the direction of a sound is greatest when the sound is in the line of axis of the two ears, and least when it is in the median plane of the body.

Now, although these statements are, generally speaking, true, yet from some experiments which I have performed I have been led to believe that they must not be considered too exactly. The experiments were as follows:—

The subject experimented upon was seated upon a chair and

blindfolded; in front was a table, and across the table was stretched a cord exactly in the median plane of the body; he was not allowed to move the head. A second cord was tied round the subject's neck below the ear. The sources of sound used were a watch and a tin whistle; and when the sound was produced the subject was asked to point in the direction of the sound; this was marked on the table, and the true direction of the sound was also marked. It was interesting to note the different degrees of accuracy with which the watch and the whistle were localised. Thus, with the watch an angle formed by the line of direction of the sound and the median plane of the body had a tangent of $\frac{29}{28}$, but was given as angle of tangent $\frac{29}{28}$; an angle of tangent $\frac{14}{28}$ was given as tangent $\frac{18}{28}$; when held in the axial line of the ears it was given as angle of tangent $\frac{19}{28}$; when held directly in the median plane of the body it was given correctly. With the tin whistle, on the other hand, all these were given correctly; but, strangely enough, an angle of tangent $\frac{29}{28}$ behind the axial line of the ears was given as tangent $\frac{9}{28}$ in front.

When one ear was closed the results of testing were very erratic, both with the watch and the whistle; thus, the whistle was sounded in the axial line of the ears, and opposite the open ears; it was given as being in the axial line of the ears, but opposite the closed ear!

From these experiments, therefore, it will be seen that the difficulty of estimating the direction of a sound directly in front of the head is not always so great as some writers have found.

It is not uncommonly stated that we are enabled to estimate the direction of a sound by the relative intensities with which it is heard by the two ears. But it must be remembered that this is only true for each particular sound, but it is incorrect to suppose that we can localise all sound with the same degree of accuracy according to the intensity with which they are heard by the two ears. Thus we may estimate the direction of a high note very accurately, while at the same distance we are not able to localise a sound of low pitch, even though the difference of intensity with which the sounds reach the two ears may be the same in both cases. Indeed, this may be said to be the crux of

the whole matter. Why are we able to localise a high note or a compound note having partial tones better than a low pure tone, a fact which experimenters are agreed upon (Mach, Lord Rayleigh, Dr P. Thompson).

At a meeting of the British Association in August 1877, Prof. S. P. Thompson read a paper in which he showed that the ear, or, rather, the two ears acting together, are enabled to distinguish differences of the phase of sound. The same fact was discovered independently by Lord Kelvin a few months later. In the *Philosophical Magazine* for June 1882, Prof. Sylvanus Thompson had a paper in which he suggested the possibility that differences of the phase with which a sound affects the two ears may aid in determining its direction.

Thanks to the kindness of Prof. MacKendrick, I was enabled to try these experiments myself. One of them was particularly interesting. I selected two ut_4 forks, and loaded one so as to give slow beats when both were sounded. They were placed several yards from one another, and a tube was led from the close proximity of one into the right ear, and another tube was led from the other fork into the left ear. As the original experimenter noticed, beats were heard very distinctly; similarly, Prof. Thompson noticed that although beats were heard yet the note seemed continuous. For my own part, I was able to observe the same strange phenomenon, but in my case it required an effort of the will; by listening very intently for the note I was enabled to hear it absolutely continuously, but on relaxing the attention it seemed interrupted by the beats. On the other hand, the beats obtruded themselves upon the mind, no matter how intently I listened for the tone and neglected them; there was no getting away from them.

Some months ago, while attempting to find out by what means the two ears acting together are able to appreciate difference of phase, I discovered a peculiar fact. Wishing to produce what may be termed a continuous positive phase in one ear while a sound was conducted to the other, I selected an intelligent individual possessed of normal hearing power. The tympanic membrane of one ear was illuminated in the usual way by reflected light and a speculum, while a tuning-fork was held a

few inches from the other ear. A fine probe was then passed into the meatus, and the centre of the tympanic membrane, with the handle of the hammer, was pushed gently inwards, not sufficiently, of course, to cause pain. According to the statement of the subject the sound of the fork was then heard considerably louder and more localised in the ear opposite which the fork was held. Thinking that this might be due to an instinctive movement of the head away from the ear which was being touched, and hence nearer to the tuning-fork, the experiment was repeated, with this difference: the fork was held vibrating opposite the end of an india-rubber tube of some length, the other end of which was inserted into the ear; thus movements of the head would not affect the distance of the ear from the source of sound. The result was the same; the sound was heard louder by one ear when the hammer of the opposite ear was pressed gently inwards.

Being surprised at the result of these experiments, a simpler and coarser one was tried. Holding the fork in one hand opposite one ear, and estimating its intensity, the other ear was then closed by the finger; again the sound was heard louder in the one ear when the opposite one was closed by pressing the finger into the meatus. According to the statement of the subject experimented on, however, the difference was not so pronounced as in the case with the probe.

The experiment was again repeated on an individual gifted with an exceptionally good ear for music; the result was the same.

Using a watch as a source of sound the same results were obtained.

It appeared to me, therefore, that there existed some reflex, hitherto undescribed, between the two ears. The next step was to find out the path of that reflex.

By good fortune I was enabled to try the experiment on an individual who had perfect hearing in the right ear, but in whom the malleus of the left ear was firmly adherent to the inner wall of the tympanum and quite immovable,—the result of suppuration many years ago. On pressing against this malleus the sound of the fork held opposite the good ear was unaffected; its

intensity did not vary in the least even with repeated experiments. From this case, therefore, it is evident that the reflex does not occur through mechanical irritation of the branches of the fifth nerve in the tympanic membrane.

The experiment was again performed upon a brother of the case just narrated. He suffered from perforation and suppuration of both ears, but with treatment the suppuration was cured; and I prevented adhesions forming, so that he hears well in spite of the perforations. When the experiments were performed on him the same results were obtained as from individuals with normal ears.

It may be concluded, therefore, that stimulation of the nerve within the labyrinth of one ear produces a change in some structure in the opposite ear which enables the latter to hear sound more acutely. The next step is to consider what change occurs in the ear opposite that in which the stimulus has been produced by pushing the malleus (and other bonelets) inwards.

Pollak (*Med. Jahrbücher*, Wien, Oct. 1892, p. 308) has shown that when one ear is stimulated by a sound the tensor tympanum of that ear and also of the opposite ear are set in action; he has further shown that this reflex is produced better by high notes than by low ones. Another fact of importance is that observed by Urbantschitsch, that stimulation of one ear by sound increases the hearing power of the opposite ear; in other words, binaural audition is more acute than monaural. Urbantschitsch attributes this fact to the passing of auditory sensation from the cerebral cortical centre of one side to that of the other, and hence the intensity of the sound appears to be greater. In the light, however, of my experiments above described, I would be inclined to attribute the increased hearing power in the case of binaural hearing to reflex action of the intrinsic muscles of the ear. In these experiments, at anyrate, the increase of the intensity of the sound cannot be due to reflex sensation from one cortical auditory centre to the other, because the stimulus applied to one ear was not auditory at all.

When we come to the question as to what structures take part in the reflex, we have two matters to consider: (1) Evidence of a similar reflex occurring between the two ears; (2) the

anatomical relationships of the auditory nerve. As regards the first of these, we have absolute evidence of a similar reflex occurring between the two ears as before stated, viz., stimulation of one ear by a sound causes contraction of the tensor tympani of both ears. It is therefore probable that this occurs in the experiments described. But we are still left to account for the fact that pressure of the chain of ossicles inwards causes increased perception for all notes, low as well as high; now contraction of the tensor tympani alone will not increase the acuteness of hearing for low notes; if anything, it will diminish this power. We must therefore seek for some other reflex besides this. I think such a reflex will be found to account for this fact, viz., that of the stapedius. We know that contraction of this muscle increases the acuteness of the hearing power, and if such a reflex could be demonstrated when the chain of ossicles is pressed inwards we would then be able to account for the whole phenomenon. Unfortunately I can find no description of such a reflex in physiological literature, and only experiment upon the living animal could give absolute proof. From the anatomical relationships of the accessory nucleus of the cochlea and the nucleus of the facial nerves before they leave the medulla, and the way in which the cochlear nerve decussates, there is certainly, anatomically speaking, a path for such a reflex. In the lower animals a reflex between the cochlea and the facial nerve does take place by this path, viz., the pricking up of the ears when the animal hears a sound; and I think it is highly probable that at the same moment a reflex action of the stapedius occurs, could we but see it. In the meantime, however, we must wait for absolute evidence, and be content at present with saying that in all probability it does occur, while the reflex action between the cochlea and the tensor tympani of both ears certainly does. The path of the reflex to the tensor tympani is as follows:—Afferent impulse from the cochlea to tuberculum acousticum, up through the lateral lemniscus to the motor nucleus of the fifth nerve; from there the efferent impulse passes to the otic ganglion, and thence to the tensor tympani.*

* To some physiologists, however, the above explanation of the facts will not appear satisfactory, because it is held by some that contraction of the

Whatever view be taken as to the causation of the facts stated above, it is evident, from them and from the experiments of Lord Kelvin and Prof. Thompson, that our lower centres, at least, are cognisant of the relative positions of the two tympanic membranes, and it has occurred to the writer that this may be one of the means by which we are enabled to estimate the direction of a sound. Thus it is evident that supposing a sound to affect two ears in different phases, the two membranes will not be in exactly the same condition at any given moment; the one may be moving in while the other is moving out, or one may be just beginning its inward movement when the other is completing it, and so on. It would not be safe, however, to assume that such a supposition is correct, unless we find facts that bear evidence in favour of its truth. Have we any such facts? I think the following may be considered as tending to corroborate the theory advanced:—

(1) If we listen to a sound conducted by water, and with our ears under the water, we lose all sense of the localities of the sound; it appears to arise in the ear. In this case, although the membranes are vibrating in and out, yet, as a whole, they have taken up a position very much further inwards than under normal conditions, because of the pressure of the water. Hence, in these circumstances, there is a state of matters in both ears similar to that which is produced in one ear by pressing in the chain of ossicles with a probe, and we are conscious of a similar

intra-tympanic muscles causes diminution of the hearing power. These physiologists would therefore not be satisfied with that explanation, even admitting that a reflex action of the muscles mentioned does occur; they would look for some other cause. I might suggest the following explanation, though, of course, it is purely speculative: When the constant inward pressure of the ossicles produces increased labyrinthine pressure, a constant stimulus passes up the auditory nerve to the nuclei in the medulla. But this stimulus is not periodic, and therefore no sound is heard. But supposing a sound to affect the opposite ear at the same time, then the two stimuli will mix in the nuclei of the medulla; and we may suppose that the constant stimulus will add to the strength of the periodic one before the latter passes on to the higher centres; that is, that a stimulus to one auditory nerve, even if it is not of such a nature as to produce a sensation of sound by itself, may yet increase the subjective intensity of a sound heard by the other ear. Of course, as above stated, such an explanation is purely speculative, and must remain so, more or less, until the nature of the nerve-current is better known than at present.

sensation in both experiments, viz., a localising of the sound in the ear.

(2) Our power of estimating the direction of a sound is much more accurate for high notes and compound tones than for low ones. Now, it may be shown by a simple mathematical process that changes in the position of the source of a sound which reaches both ears will in general produce greater alterations in the phase with which it strikes the two ears when the note is high than when it is low.

(3) Prof. Thompson has shown that the two ears do appreciate a difference in the nature of the sound when the phases are always exactly opposite in the two ears; and it is not a very great step to suppose that our ears will perceive differences when one tympanum is repeatedly, though not always, moving in a direction opposite from the other.

It must not be supposed that this perception of the difference phase by the two ears is necessarily an auditory sensation. Indeed, in some experiments its most prominent feature is not of that nature, *e.g.*, if the sound of a tuning-fork is led to the two ears in such a way that the phases are exactly opposite, the sound is, in its auditory characters, pitch etc., the same as in ordinary hearing, but it seems to come from the middle of the head; thus the peculiar feature in this case is its locality, not its auditory character.

Knowing, therefore, that by the aid of two ears we are enabled to perceive differences of phase, and having seen that this perception is not necessarily auditory, it remains to consider by what faculty we do perceive the difference.

In the middle ear there are two muscles, the tensor tympani and the stapedius, and they are inserted into the malleus and stapes respectively in such a way that with a movement inwards of the membrana tympani the tensor is slightly relaxed and the stapedius is rendered tense, and, conversely, outward movement of the membrane produces an opposite result in each muscle. By this means, therefore, it would appear that we have the faculty of appreciating the direction of movement of the tympanic membrane occurring simultaneously in the two ears; or, to put it in other words, we may be able to perceive variations of

the phase of sound-waves which reach the two membranes simultaneously by means of the muscular sense.

If this view of the matter therefore be correct, it is evident that we will be assisted in judging of the direction of a sound by means of the muscular sense. And further, it is probable that not only may this sense give us information of the difference of phase in the two ears, but it may also help us in estimating differences of intensity in the sound as heard by the two ears, for it is plain that in that ear in which the sound is most intense the strain put upon the muscles will be greater than in the ear in which the sound is less loud.

Thus, according to the explanation advanced above, the senses of hearing and sight have this in common, that the means by which we estimate direction resides in the muscular sense.

Of course, other characteristics of sound help us in estimating its direction: differences of intensity and differences of quality with which it is heard by the two ears. The latter characteristic, quality, will be of especial value in enabling us to judge the direction of sounds with which our ear is well acquainted, and is thus for the most part associative. These, however, are outside the range of the present paper.

Before closing I would like to point out a matter in which the experiments described above might be put to practical use. It was shown that when the chain of bones was pressed gently inwards, a tuning (or watch) held opposite the other ear was heard louder; and it was shown, further, that this was due to a stimulus passing from the labyrinth of that ear. Now, in a condition such as fixation of the stapes in the fenestra ovalis, it is obvious that this reflex could not occur. The experiment might therefore be used for the diagnosis of such a condition. On *a priori* grounds it would appear to be a good negative test, but not a valuable positive one. That is to say, if a patient heard the sound better in the opposite ear during the inward pressure of the chain of ossicles, then we could safely assume that the stapes was not fixed. But the converse might not always be true, because the subject might not notice any difference, even though the stapes were free.

As this affection is usually difficult of diagnosis, the test

suggested deserves a trial. The subject, however, is one for surgical consideration.

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On the Geometrical Investigation of the Circular Functions of 3θ and 5θ. By Prof. A. H. Anglin.

(Read May 3, 1897.)

1. Let the $<$PAB be θ, where P is a point on the arc of a semicircle described on AB of which O is the middle point. Then making

$$<\mathrm{OPC} = <\mathrm{OPA}, \text{ the } <\mathrm{PCB} = 3\theta.$$

Since the triangles APC, OPC are similar, we have

$$\frac{\mathrm{AC}}{\mathrm{OC}} = \frac{\Delta\,\mathrm{APC}}{\Delta\,\mathrm{OPC}} = \frac{\mathrm{AP}^2}{\mathrm{OP}^2} = \frac{\mathrm{AP}^2}{\mathrm{AO}^2}.$$

We shall express each of the ratios $\sin 3\theta/\sin\theta$, $\cos 3\theta/\cos\theta$, etc., as a function of AC/OC.

$$\sin 3\theta = \frac{\mathrm{MP}}{\mathrm{PC}} = \frac{\mathrm{MP}}{\mathrm{AP}} \cdot \frac{\mathrm{AP}}{\mathrm{PC}} = \frac{\mathrm{MP}}{\mathrm{AP}} \cdot \frac{\mathrm{AO}}{\mathrm{OC}}.$$

Thus

$$\sin 3\theta/\sin\theta = \frac{\mathrm{AO}}{\mathrm{OC}} = \frac{\mathrm{AC}}{\mathrm{OC}} - 1$$

$$= 4\left(\frac{\mathrm{AP}}{\mathrm{AB}}\right)^2 - 1 = 4\cos^2\theta - 1 = 3 - 4\sin^2\theta.$$

$$\cos 3\theta = \frac{\mathrm{CM}}{\mathrm{CP}} = \frac{\mathrm{AM}}{\mathrm{CP}} - \frac{\mathrm{AC}}{\mathrm{CP}}$$

$$= \frac{\mathrm{AM}}{\mathrm{AP}} \cdot \frac{\mathrm{AP}}{\mathrm{CP}} - \frac{\mathrm{AP}}{\mathrm{AO}} = \frac{\mathrm{AM}}{\mathrm{AP}} \cdot \frac{\mathrm{AO}}{\mathrm{OC}} - 2\frac{\mathrm{AP}}{\mathrm{AB}}.$$

Thus

$$\cos 3\theta/\cos\theta = \frac{\mathrm{AO}}{\mathrm{OC}} - 2 = \frac{\mathrm{AC}}{\mathrm{OC}} - 3$$

$$= 4\left(\frac{\mathrm{AP}}{\mathrm{AB}}\right)^2 - 3 = 4\cos^2\theta - 3$$

$$\tan 3\theta = \frac{\mathrm{PM}}{\mathrm{CM}} = \frac{\mathrm{PM}}{\mathrm{CP}} \Big/ \frac{\mathrm{CM}}{\mathrm{CP}}$$

$$= \frac{\mathrm{PM}}{\mathrm{AM}} \cdot \frac{\mathrm{AO}/\mathrm{OC}}{\mathrm{AO}/\mathrm{OC} - 2}.$$

Thus

$$\tan 3\theta/\tan\theta = \frac{AC/OC - 1}{AC/OC - 3} = \frac{AP^2 - AO^2}{AP^2 - 3AO^2}$$
$$= \frac{3AP^2 - BP^2}{AP^2 - 3BP^2} = \frac{3 - \tan^2\theta}{1 - 3\tan^2\theta}.$$

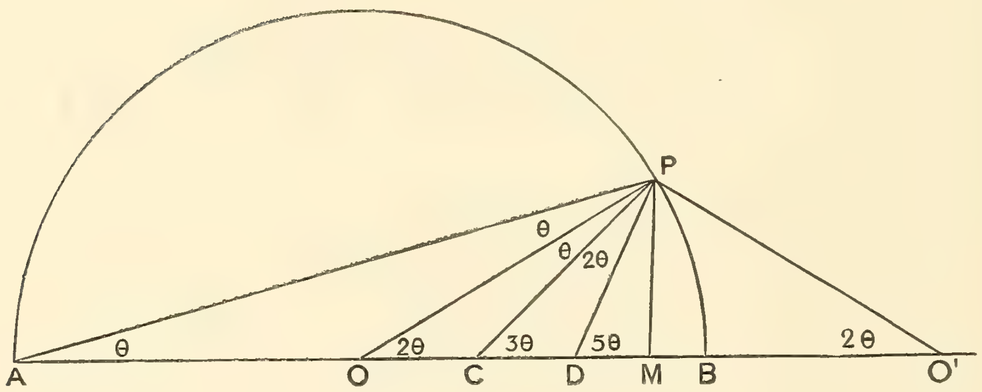
2. We may also obtain $\tan 3\theta$ without reference to the work for sine and cosine.

$$\tan 3\theta = \frac{PM}{AM - AC} = \frac{PM/AM}{1 - AC/AM}.$$

Producing OM so that $MO' = MO$, the triangle $AO'P$ is similar to the triangles APC and OPC. Then

$$\frac{AO}{OC} = \frac{AP}{PC} = \frac{AO'}{O'P} = \frac{AO'}{AO}$$

$$\therefore \quad \frac{AC}{OC} = \frac{2AM}{AO}, \quad \text{and} \quad \frac{AC}{AM} = \frac{2OC}{AO}.$$



Thus

$$\tan 3\theta/\tan\theta = \frac{AO}{AO - 2OC} = \frac{AC - OC}{AC - 3OC}, \text{ as before.}$$

3. The solution may also be expressed without reference to the relation

$$\frac{AC}{OC} = \frac{AP^2}{AO^2}.$$

For

$$\sin^2\theta = \frac{MB}{AB}, \ \cos^2\theta = \frac{AM}{AB}, \ \tan^2\theta = \frac{MB}{MA};$$

and

$$\frac{AC}{OC} = \frac{AO}{OC} + 1 = \frac{2AM}{AO} = 4\frac{AM}{AB}.$$

Thus

$$\sin 3\theta/\sin\theta = \frac{AC}{OC} - 1 = 3 - 4\frac{MB}{AB} = 3 - 4\sin^2\theta\,.$$

$$\cos 3\theta/\cos\theta = \frac{AC}{OC} - 3 = 4\frac{AM}{AB} - 3 = 4\cos^2\theta - 3\,.$$

$$\tan 3\theta/\tan\theta = \frac{AC/OC - 1}{AC/OC - 3}$$
$$= \frac{3MA - MB}{MA - 3MB} = \frac{3 - \tan^2\theta}{1 - 3\tan^2\theta}\,.$$

4. Making

$$\angle CPD = \angle APC, \text{ the } \angle PDB = 5\theta\,.$$

Since the triangles OPD, CPD are similar, we have

$$\frac{OD}{CD} = \frac{\Delta OPD}{\Delta CPD} = \frac{OP^2}{CP^2}\,.$$

We shall express each of the ratios $\sin 5\theta/\sin\theta$, $\cos 5\theta/\cos\theta$, etc., as a function of AC/OC.

$$\sin 5\theta = \frac{MP}{DP} = \frac{MP}{AP}\cdot\frac{AP}{DP} = \frac{MP}{AP}\cdot\frac{AC}{CD}\,.$$

Now

$$\frac{AC}{CD} = \frac{AC}{OC}\cdot\frac{OC}{CD} = \frac{AC}{OC}\left(\frac{OD}{CD} - 1\right),$$

and

$$\frac{OD}{CD} = \frac{OP^2}{CP^2} = \frac{AO^2}{AC\,.\,OC}, \quad \text{since } \frac{AC}{CP} = \frac{CP}{OC}$$
$$= \frac{(AC - OC)^2}{AC\,.\,OC} = \frac{AC}{OC} + \frac{OC}{AC} - 2\,.$$

Thus

$$\sin 5\theta/\sin\theta = \left(\frac{AC}{OC}\right)^2 - 3\frac{AC}{OC} + 1$$
$$= \left(\frac{AP}{AO}\right)^4 - 3\left(\frac{AP}{AO}\right)^2 + 1$$
$$= 16\cos^4\theta - 12\cos^2\theta + 1 = 5 - 20\sin^2\theta + 16\sin^4\theta\,.$$

$$\cos 5\theta = \frac{DM}{DP} = \frac{AM}{DP} - \frac{AD}{DP}\,.$$

Now

$$\frac{AM}{DP} = \frac{AM}{AP}\cdot\frac{AP}{DP} = \cos\theta\,.\,\frac{AC}{CD}\,;$$

and

$$\frac{AD}{DP}=\frac{AC}{CP}\cdot\frac{\sin APD}{\sin APC}=\frac{AP}{AO}\cdot\frac{\sin OPO'}{\sin POO'}$$

$$=2\cos\theta\,.\,\frac{OO'}{AO}=2\cos\theta\left(\frac{AC}{OC}-2\right).$$

We may also obtain AD/DP by strict geometry :

$$\frac{AD}{DP}=\frac{AO}{DP}+\frac{OD}{DP}=\frac{CP}{CD}+\frac{AO}{CP},\text{ by similar triangles,}$$

$$=\frac{CP}{OC}\cdot\frac{OC}{CD}+\frac{AC}{CP}\cdot\frac{AO}{AC}=\frac{AP}{AO}\left(\frac{OC}{CD}+\frac{AO}{AC}\right)$$

$$=2\cos\theta\left(\frac{AC}{OC}-2\right).$$

Thus

$$\cos 5\theta/\cos\theta=\frac{AC}{CD}-2\frac{AC}{OC}+4$$

$$=\left(\frac{AC}{OC}\right)^2-5\frac{AC}{OC}+5$$

$$=16\cos^4\theta-20\cos^2\theta+5\,.$$

$$\tan 5\theta=\frac{PM}{DM}=\frac{PM}{DP}\Big/\frac{DM}{DP}$$

$$=\frac{PM}{AM}\cdot\frac{AC}{CD}\Big/\left(\frac{AC}{CD}-2\frac{AC}{OC}+4\right).$$

Thus

$$\tan 5\theta/\tan\theta=\frac{AC^2-3AC\,.\,OC+OC^2}{AC^2-5AC\,.\,OC+5OC^2}$$

$$=\frac{5AP^4-10AP^2\,.\,BP^2+BP^4}{AP^4-10AP^2\,.\,BP^2+5BP^4}$$

$$=\frac{5-10\tan^2\theta+\tan^4\theta}{1-10\tan^2\theta+5\tan^4\theta}.$$

If we express the solution by reference to the relation AC/OC = 4AM/AB, we shall have

$$\tan 5\theta/\tan\theta=\frac{5AM^2-10AM\,.\,BM+BM^2}{AM^2-10AM\,.\,BM+5BM^2}$$

$$=\frac{5-10\tan^2\theta+\tan^4\theta}{1-10\tan^2\theta+5\tan^4\theta}.$$

The Antivenomous Properties of the Bile of Serpents and other Animals; and an Explanation of the Insusceptibility of Animals to the Poisonous Action of Venom introduced into the Stomach. By Professor Fraser, M.D., LL.D., F.R.S.

(Read July 5, 1897.)

(*Abstract.*)

In a paper communicated to this Society I stated that, when introduced into the stomach of an animal, serpents' venom produces no obvious injury, even when the quantity is so large as to be sufficient to kill 1000 animals of the same species and weight if the venom were injected under the skin.* The failure of so highly toxic a substance to produce poisoning when it is administered by the stomach might be due to chemical changes produced upon it by the secretions of the stomach and intestines, or to non-absorbability into the blood from the stomach and intestines. It is already known that the toxicity of venom is not materially reduced by gastric digestion. Although at first sight incompatible with the innocuousness of stomach administration, this fact is not in reality a contradiction of it, for the absorbing power of the stomach for many organic substances—even for strychnine—has been shown to be extremely slight, and their entrance into the circulation appears to occur not in the stomach but in the intestines.

As serpents' venom introduced into the stomach is not rendered innocuous by the stomach secretions, while, notwithstanding, it fails to cause poisoning, it may be assumed that the stomach walls are incapable of absorbing it. If, like other poisons, it can be absorbed from the intestines, the explanation of the failure to produce toxic symptoms when it is administered by the stomach might depend on a chemical or physiological destruction of its toxic properties by some substance or substances which it encoun-

* *Proceedings of the Royal Society of Edinburgh*, vol. xx., 1894–5, p. 471.

ters soon after entering the intestinal canal, and most probably, therefore, by the bile or the pancreatic secretion.

To fully explain the innocuousness of stomach administration would accordingly require that the effects on venom of the biliary and other intestinal secretions should be investigated, and also the absorbability of venom through the intestinal walls.

As a contribution to the settlement of the question, I have made a number of experiments with the biliary secretion; and whatever may be the influence of the other secretions or of intestinal absorption, that of the bile has been found to be so decided as to be in itself sufficient to account for the innocuousness of stomach administration.

The bile from the gall-bladder of the African cobra, puff-adder, rattlesnake, and grass snake was used, and each bile was tested against the venom of the African and the Indian cobra.* For the most part, the experiments were made by mixing various quantities of each bile with a little more than the minimum-lethal dose of the venom, and then injecting this mixture under the skin of the animal. The object of the experiments was not only to determine if the bile can render venom innocuous, but also, if it have this power, what is the smallest quantity of bile capable of doing it. As bile varies much in its concentration, it was always weighed in the dry form, so as to remove avoidable fallacies in dosage and in any comparison that might be made between the bile of different animals.

Taking first the venom of the African cobra, its minimum-lethal dose for rabbits was determined and found to be ·000245 grm. p. kilo., but a slightly larger dose—viz., ·00025 grm.—was taken as the minimum-lethal.

The experiments were made by mixing together and leaving in contact with each other for ten minutes this dose of venom with a dose of bile, each substance having been dissolved in a few 10ths

* For supplies of dried serpents' bile I am indebted to Mr J. W. van Putten, of Brakfontein, South Africa, who has sent me (April and June 1895, and December 1896) the bile of the African cobra, puff-adder, and night-adder; and also to Mr J. H. V. Payne, of Hout Bay, Cape Colony, from whom I have received (December 1896) the bile of the puff-adder. The rattlesnake bile was taken by me from a living snake, more recently presented by Professor Malcolm Laurie, of St Mungo's College, Glasgow.

of a c.c. of water, and then injecting the mixture under the skin of the animal.

When this was done with the bile of the same serpent, the African cobra, death occurred when the dose of bile was ·00008 grm. p. kilo. or less, but recovery when the quantity was ·0001, ·00015, ·0002, or ·0005. With rattlesnake bile, death occurred when the quantity of bile was ·00025 grm. p. kilo. or any smaller quantity, but recovery when it was ·0003, ·00035, ·0004, or ·0005 grm. p. kilo. With puff-adder bile, death occurred when the dose was ·0005 or ·00075 grm. p. kilo., but recovery when it was ·001, ·002, ·0025, or ·003 grm. p. kilo.

To render the evidence still more obvious, experiments were also made with a larger dose of African cobra venom, viz., ·00026 grm. p. kilo.,—a dose which is fatal to a rabbit in less than eight hours. When mixed with the bile of the same serpent, ·0025, ·00225, ·002, and ·00175 grm. p. kilo. was each sufficient to prevent death, which, however, followed when the quantity was ·0015 grm. p. kilo. or a smaller quantity. When mixed with the bile of the puff-adder, ·003 or ·0025 grm. p. kilo. was sufficient to render this dose of venom non-lethal, but ·002 and ·001 grm. p. kilo. were insufficient.

Similar experiments were made with the venom of the Indian cobra, but while the dose taken as the minimum-lethal was the same as in the first series of experiments with the venom of the African cobra—viz., ·0025 grm. p. kilo.—this dose really represented a larger excess over the minimum-lethal. The results were that rattlesnake bile in a dose of ·00025 grm. p. kilo. was insufficient to render this dose of Indian cobra venom non-lethal, but was sufficient in a dose of ·0005, ·0004, ·00035, or ·0003 grm. p. kilo.; that African cobra bile was insufficient in doses of ·00075, ·0005, ·0004, ·0003, and ·00025 grm., but sufficient in doses of ·002 and ·001 grm. p. kilo.; and that puff-adder bile was insufficient in doses of ·001, ·002, ·0025, and ·00275, but sufficient in doses of ·00325 and ·003 grm. p. kilo.

It was thus shown that the bile of venomous serpents is able, when mixed with the venom of serpents, to prevent lethal doses of the latter from producing death; and that the bile is indeed so powerful an agent in doing this that a quantity actually smaller than the quantity of venom may be sufficient for the purpose. It

need scarcely be added that the doses of bile thus shown to be sufficient represent only minute portions of the bile stored in the gall-bladder of a serpent, and that a serpent, therefore, has at its disposal enough of bile to prevent injury from venom introduced into the stomach in quantities many times greater than the minimum-lethal.

It appeared of interest to examine in a similar manner the bile of innocuous serpents and of other animals.

All serpents, innocuous as well as venomous, exhibit a resistance against the toxic action of venoms introduced subcutaneously or directly into the circulation, which is not dependent on their being cold-blooded animals. Various facts, some of which are anatomical, tend to show that the innocuous, equally with the venomous serpents, possess poison glands and secrete venom. The former are innocuous only in the sense that they are not normally furnished with weapons of offence in the form of poison fangs. Most probably, therefore, the relative protection against the poisonous action of venom introduced into the circulation, which is common to serpents, is dependent upon an effect produced upon them by the venom which they all secrete, although in the case of the innocuous serpents only in relatively small quantities.

Experiments that were made with the bile of the innocuous grass snake have confirmed this supposition. When tested in rabbits against ·00025 grm. p. kilo. of the venom of the Indian cobra, ·002, ·0045, and ·0055 grm. p. kilo. were found insufficient to prevent death, but ·0065, ·0075, and ·01 grm. p. kilo. was each found to be able to prevent death. The smallest quantity of the bile of this innocuous serpent necessary to prevent death is therefore considerably larger than that of the feeblest of the biles of venomous serpents, but still is only a small quantity.

Experiments were also made with the bile of the ox, as representing those animals which do not secrete venom or possess any protection against venom introduced into the subcutaneous tissues or into the circulation, while at the same time they are unaffected by venom introduced into the stomach. It was found that when mixed with ·00025 grm. p. kilo. of the Indian cobra venom, this bile failed in rabbits to prevent death in doses of ·01 and ·015 grm. p. kilo., but succeeded in doses of ·02, ·03, ·04, ·07, and ·15

grm. p. kilo. The bile of the ox, therefore, is able to antagonise the toxic action of serpents' venom, its antagonising power, however, being only about the one-seventieth of that of the strongest of the biles of venomous serpents that have been tested. It was shown by other experiments that the bile of the rabbit and of the guinea-pig also possesses this anti-venomous property, and also in a degree which, though feeble when compared with the bile of venomous serpents, is yet in itself considerable.

While, therefore, it may now be assumed that the bile of all animals is anti-venomous, the difference observed between the potency of the bile of venomous serpents when contrasted with that of ordinary animals suggests that the anti-venomous property must be dependent, at least in part, on some specific constituent or constituents present in different quantities in the bile of different animals. As this constituent is most largely present in the bile of venomous serpents, an endeavour was made to separate it from their bile. For this purpose, absolute alcohol was added in excess to a concentrated solution of the bile of the puff-adder, of which I possessed a greater quantity, though still only small, than that of any other venomous serpent. By this means, the substances soluble in alcohol, consisting of the bile salts and of a small quantity of bile pigments, were separated in solution from the substances insoluble in alcohol, consisting of proteids and of the bulk of the bile pigments, and it was hoped also of the special anti-venomous constituent. Water was then added to the substances insoluble in alcohol, and the watery solution, after having been centrifugalized, was evaporated to dryness over sulphuric acid in the vacuum of an air pump. The alcoholic solution was also dried, in the first place on a water bath at 100° F., and afterwards *in vacuo* over sulphuric acid. This analysis was made with the whole of the bile at my disposal for the purpose, which, however, weighed only ·5 grm., and from it ·45 grm. of substances soluble in alcohol and ·05 grm. of those insoluble were obtained. The extraction of the latter with water yielded only ·02 grm. of a dark green solid, representing, therefore, about the one-twenty-sixth part of the original bile.

Several experiments were made with each of the two products. When the product consisting of the substances soluble in alcohol

was tested against the minimum-lethal dose of Indian cobra venom, it was found to be incapable of preventing death in rabbits when the doses of it were ·02, ·01, ·005, ·004, and ·0027 grm.; and in white rats when the doses were ·001, ·00045, and ·00027 grm. p. kilo. The anti-venomous quality of this bile, therefore, is not appreciably, if at all, dependent on the constituents soluble in alcohol, which include all the bile salts.

When, on the other hand, the part of the alcohol precipitate soluble in water was tested in white rats against the minimum-lethal dose of the same venom, while death occurred with ·000008 grm. p. kilo., recovery occurred with ·00001, ·00002, ·000025, and ·00003 grm. p. kilo. As the quantity of this product was very small, the experiments were restricted to white rats, for which animals the minimum-lethal dose of Indian cobra venom is slightly less than ·0003 grm. p. kilo., and the smallest quantity of the dried natural bile of the puff-adder required to prevent death from this dose of venom is ·00025 grm. p. kilo. The extremely small quantity of ·00001 grm. of this bile product is, therefore, capable of preventing death in the same circumstances as ·00025 grm. of the original bile.

The isolation of the anti-venomous constituent made it possible to obtain further and different evidence of the power of bile to render venom inert. Although the bile in its original form possesses much power in antagonising venom when the two substances are mixed together *in vitro*, only ·0003 grm. of rattlesnake bile or ·02 grm. p. kilo. of ox bile being required to render a minimum-lethal dose of Indian cobra venom innocuous, it cannot be too distinctly stated—for the facts are also applicable to the antidotism between disease toxines and anti-toxines—that these data are apt to give an exaggerated conception of the curative value of the antidote when it is administered after the venom. Generally speaking, even when no more than the minimum-lethal dose of venom is used, the quantity of the antidote required is from 1600 to 2000 times greater in the latter than in the former condition of administration. Although, while in the alimentary canal, bile is non-toxic, it is altogether different when it is injected under the skin or into a blood-vessel. The bile salts and the bile pigments then act as poisons; and if a dose of bile were injected

under the skin containing a sufficient quantity of the anti-venomous constituent to antagonise a minimum-lethal dose of venom received half-an-hour previously, the constituents of the bile which are non-antidotal, but at the same time toxic, might be so great in amount as to produce death. Thus, in an experiment made with African cobra bile, administered, in a dose estimated to be sufficient, thirty minutes after a minimum-lethal dose of cobra venom had been injected subcutaneously, the animal survived for four days; whereas an animal used in a control experiment without bile died in six hours. During the two days before death, the animal which had received both bile and venom exhibited symptoms, however, which were rather those of bile than of venom poisoning.

It is improbable, therefore, that the bile in its natural form could be used as an antidote except by stomach administration or by application to the wound caused by a snake-bite.

The successful result of the attempt made to isolate its antidotal constituent has, however, rendered it possible to test the therapeutic value of this constituent when it is introduced into the blood of an animal which had already received a lethal dose of venom. The quantity at my disposal being only small, no more than one experiment could be made; and for the same reason, it was made on a white rat. Thirty minutes after the animal had received by subcutaneous injection ·0003 grm. p. kilo. of Indian cobra venom, ·012 grm.—equivalent to ·075 grm. p. kilo.—of the part soluble in water of the alcoholic precipitate of puff-adder bile was injected under the skin of the opposite side of the body. Only slight symptoms of the action of the venom were manifested, consisting chiefly of loss of appetite and indisposition to go about; but they had completely disappeared at the end of twenty-four hours. In a control experiment, a white rat of almost the same weight received by subcutaneous injection a part of the same solution of Indian cobra venom, representing also a dose of ·0003 grm. p. kilo. Grave symptoms were produced in a few hours, and the animal was dead on the following day.

This experiment, taken in conjunction with the considerable number of *in vitro* experiments that have been made, not only supplies strong confirmation of the evidence given by the latter experiments that bile is able to render serpents' venom inert, but

also suggests that from bile there may be produced an antidote for snake poisoning, which, in its antidotal value, is at least equal to the most powerful antivenene or antivenomous serum as yet obtained from the blood of immunised animals.

It is interesting to find that serpents' bile enters into the composition of the medicines most trusted in for the treatment of snake-bite by the natives of Africa. Several specimens of native snake medicines have been sent to me, which contain bile along with many other ingredients, such as serpents' heads, dried and powdered with the retained venom glands. As I have stated in a former communication, venom itself is used by stomach administration as an antidote, but many "snake doctors" consider that bile is more effective. It is employed by them not only by stomach administration but also by rubbing it into the wound inflicted by the serpent. A "snake doctor," with a high local reputation, informed one of my correspondents that bile is the better antidote, but that, as venom can be more largely obtained, he uses it in the first instance, and reserves the bile for employment should the recovery of the patient appear doubtful. When asked to assist in procuring serpents' gall-bladders in order that the bile might be examined, he hesitated, and then exclaimed, "Oh! they are beginning to learn too much;" being obviously alarmed lest his practice should be injured.

The interest associated with this action of bile is probably not restricted to its effect on venoms. Analogies in composition would suggest that the same action is exerted upon the toxins of disease, a suggestion which also receives support from the further analogy that many of these toxins—such as those of tetanus and diphtheria—while notoriously active when present in the blood, are inert when introduced into the stomach. Even when circulating in the blood, toxins, in common with other organic poisons, are probably being constantly eliminated into the alimentary canal, where they would at once lose their destructive power by coming into contact with bile, and thereby the total effective quantity of the toxin in the body would be reduced. There are also many poisonous substances generated in the intestinal canal, which in some circumstances produce poisoning. The production of this auto-intoxication may partly be due to the failure of the liver to secrete a sufficient amount

of healthy bile ; and the therapeutic value of cholagogue substances, so long employed on empirical grounds, may thus also obtain a satisfactory explanation.

Bile is produced in large quantity by all animals, and it is present in the intestinal canal throughout its whole length ; but beyond the fact that it is an important excretion, removing waste products from the body, its functions are not known to be of high value. So much is this the case, that a recent authority has remarked, "the bile is a most elaborate secretion ; it is poured into the intestine, and finds apparently little to do." * To such recognised functions of the bile as the promotion of fat absorption and those dependent upon its laxative and indirect antiseptic properties, there may now be added the additional function of rendering inert many organic poisons introduced into or produced in the alimentary canal.

* Halliburton : *A Text-book of Chemical Physiology and Pathology*, 1981, 687.

On the Electrification of Air by Uranium and its Compounds. By J. Carruthers Beattie, D.Sc. *With a Note by* Lord KELVIN, G.C.V.O., F.R.S., etc. etc.

(Read June 7, 1897.)

§ 1. It is proposed in the following paper to describe experiments made to test the electric state of the air in the neighbourhood of metallic uranium, or of other metals on which a salt of uranium had been deposited from a solution, when these were charged to a positive or a negative potential.

§ 2. Method employed. To test the electric state of the air the electric filter method* due to Kelvin, Maclean, and Galt was employed. The special filter used in the experiments to be described was a block-tin pipe, 10 cms. long and 1 cm. diameter, filled with brass filings. This was insulated on two tunnelled pieces of paraffin, and put in metallic connection with the insulated pair of quadrants of a quadrant electrometer. From one of the tunnelled pieces of paraffin a metal tube led to an air pump; from the other a piece of india-rubber tubing led to the place where the air to be tested was. This air was then drawn through the electric filter, and the deviation from the metallic zero of the electrometer, when the two pairs of quadrants were insulated, was noted. To give some idea of the efficiency of the filter, the results obtained when air was pumped away from the neighbourhood of the electrodes of a Ruhmkorff inductorium (Apps. pattern, 10-inch spark) will be given.

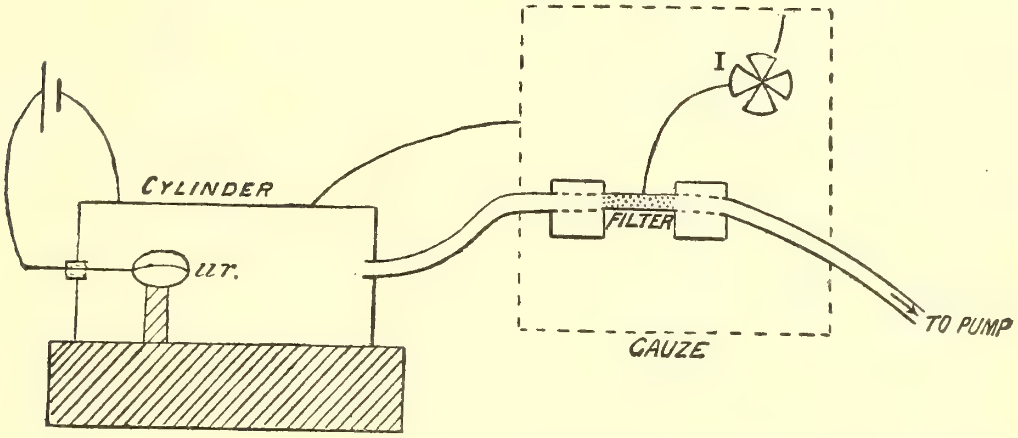
When the electrodes were near enough to admit of sparks passing it was found that air, drawn from the immediate neighbourhood of the positive electrode, gave a positive electrification of $+1·5$ volts on the electrometer after one minute of pumping at the rate of twelve strokes per minute. When the air was drawn from the immediate neighbourhood of the negative electrode, a negative electrification of $-2·5$ volts in half a minute was observed.

* *Proc. Roy. Soc. (London)*, March 14, 1895.

When the electrodes were drawn so far apart that sparking did not take place, air drawn from the positive electrode raised the filter to a potential of $+\cdot 7$ of a volt after one minute's pumping; air drawn from the neighbourhood of the negative electrode gave a reading of $-1\cdot 5$ of a volt in the same time.

When the negative electrode was connected to the cathode of a Crookes' tube, air drawn from the immediate neighbourhood of the cathode through the filter caused an electrometer deviation equivalent to a potential of $-\cdot 8$ of a volt. Air drawn from the immediate neighbourhood of the wire connecting the negative electrode to the cathode caused a deviation equivalent to $-\cdot 7$ of a volt. Air drawn from the neighbourhood of the anode or of the wire connecting the anode to the positive terminal of the coil gave a positive reading on the electrometer less in amount than the corresponding negative reading.

§ 3. Electrification of air by metallic uranium. To examine the



electric state of the air in the neighbourhood of metallic uranium charged to a given potential, the following experimental arrangement was made. A metallic cylinder, 24 cms. length, 8 cms. diameter, was constructed. One end of this was closed with a piece of the same metal; in this end was a hole into which a plug of tunnelled paraffin was placed; from the paraffin a piece of india-rubber tubing about $1\frac{1}{2}$ feet long led to the electric filter. The other end of the metallic cylinder was closed with a disc of paraffin, in the centre of which a hole was made to admit air from the laboratory. This hole was filled with cotton wool. The metallic uranium, $5\cdot 5$ cm. diameter, $\cdot 5$ cm. thickness, was insulated on paraffin inside the cylinder. The cylinder itself was laid on a

block of paraffin. The arrangement will easily be understood from the diagram on p. 467.

In the first instance a piece of lead of the same size as the uranium was insulated on paraffin in the metallic cylinder, and was connected to the case of the electrometer. The metal cylinder was also connected to the electrometer case. Air drawn from the cylinder through the filter caused no deviation from the metallic zero of the electrometer. When the lead was disconnected from the case of the electrometer and joined to one terminal of a battery and the other terminal connected to the case of the electrometer no electrification of the air was observed with the lead at potentials up to ± 95 volts.

When the uranium was insulated in the metallic cylinder, and this latter was of copper polished inside, the following results were obtained.

With uranium and the copper cylinder connected to case of electrometer a positive electrification of the air was observed, such that an electrometer deviation of $+\cdot05$ of a volt was produced per minute, the pump being worked at the rate of twelve strokes per minute. The uranium was disconnected from the electrometer case and joined to one terminal of a battery, the other terminal and the copper cylinder being connected to the case of the electrometer. The results are given in the following table :—

Uranium kept at	Electrometer reading per minute of pumping. Twelve strokes per minute.
+ 2 volts.	+ ·36 of a volt.
+ 10 ,,	+ ·65 ,,
+ 22 ,,	+ ·53 ,,
+ 45 ,,	+ ·35 ,,
+ 68 ,,	+ ·25 ,,
+ 93 ,,	+ ·235 ,,
− 2 ,,	− ·32 ,,
− 10 ,,	− ·53 ,,
− 22 ,,	− ·40 ,,
− 45 ,,	− ·25 ,,
− 68 ,,	− ·22 ,,
− 93 ,,	− ·20 ,,

The uranium and one terminal of the battery were next con-

nected to the case of the electrometer, and the copper cylinder joined to the other terminal. With this arrangement it was found that there was a negative electrification of the air with the positive terminal connected to the copper cylinder, and a positive electrification with the negative terminal connected to the same cylinder; the amounts of these electrifications per minute were exactly equal to the electrification of the same sign when the uranium was connected to the battery and the cylinder to the case. Thus, with the cylinder at $+22$ volts, a negative electrification of the air was produced, which gave a deviation on the electrometer per minute of pumping equivalent to $-\cdot 40$ of a volt.

The same results were obtained after the inside surface of the copper cylinder had been oxidised.

With the uranium insulated in a zinc cylinder and with uranium and cylinder connected to case, an electrometer reading equal to $-0\cdot 1$ of a volt per minute of pumping was observed. With an aluminum cylinder and the same connections a slight positive electrification of the air was observed. With a lead cylinder a negative electrification of $-\cdot 05$ of a volt per minute was found.

The results obtained when the uranium was kept at a definite potential and the cylinder connected to case were the same as in the experiments with the copper cylinder. That is, the nature of the metal of the vessel surrounding the uranium did not influence the extent to which the air was electrified.

In all cases the air in the metallic cylinder was electrified positively when the uranium was electrified positively, negatively when the uranium was electrified negatively, and always, for a given positive potential of the uranium, the air was electrified more strongly positive than it was negative for a like negative potential of the uranium. The electrification of the air attained a maximum when the uranium was charged to a potential between ± 10 and ± 22 volts.

The uranium was next wrapped in aluminium foil and again insulated in the lead cylinder. The air drawn away from the cylinder was found to be negatively or positively electrified according as the aluminium-foiled uranium was negatively or positively electrified. The maximum electrification of air was obtained with the covered uranium charged to about 6 volts. The

effects were less than those obtained with the uranium alone. For example, with the aluminium-foiled uranium kept at + 2 volts and the lead cylinder to case; or with first to case and cylinder at – 2 volts, the electrification of the air drawn through the filter caused an electrometer deviation of + ·11 of a volt per minute of pumping. With + 10 volts the electrometer deviation was + 0·05 per minute with + 95, + ·015 of a volt per minute. The negative electrifications of the air with the uranium kept at – 2, – 10, or at 95 volts and the metallic cylinder to the case of the electrometer were smaller than the corresponding positive electrification.

Finally, the uranium was placed inside a paraffin cylinder. Round the outside of this cylinder tinfoil was wrapped. With both uranium and tinfoil connected to case, the air drawn from the cylinder showed no signs of electrification. With the uranium positively or negatively electrified, the air was found to be positively or negatively electrified. The amount of the electrification for any given voltage depended, however, on the order in which the voltages were taken. For example, after the tinfoil wrapped round the paraffin cylinder had been connected to the positively electrified terminal of the battery and the uranium to case, it was found that the air drawn through the filter caused an electrometer deviation of – ·235 of a volt per minute. In the succeeding six minutes, while the tinfoil was kept at 10 volts and the uranium connected to case, the electrometer deviation was + ·7 of a volt per minute. In the next two minutes it was + ·6 of a volt per minute, and six minutes later it was + ·3 of a volt. Both uranium and tinfoil were next connected to case, and in the first two minutes there was an electrometer deviation of – ·6 of a volt per minute of pumping. In the second two minutes this deviation fell to – ·13 of a volt per minute. With the uranium connected to case and the tinfoil kept at a potential of + 10 volts, the air was found to be negatively electrified, the amount of this electrification decreased in 10 minutes from – ·65 of a volt in the first minute to – ·2 of a volt in the tenth minute. On again connecting both uranium and tinfoil to case, the air was found to be + ly electrified: the deviation obtained in the first minute was + ·65 of a volt, in the 8th + ·15 of a volt.

§ 4. Electrification of air by uranium salts.—Two salts of

uranium were used—uranium acetate and uranium nitrate. The nitrate was deposited on a strip of platinum. The platinum was then insulated on paraffin in the copper cylinder described above. The same series of experiments were gone through, and results of the same nature as those described in § 3 were obtained. The electrifications of air observed were, however, much smaller in amount. The acetate was deposited on tinfoil, and placed in a zinc cylinder which was provided with a mica window. By means of this window ultra violet light from an arc lamp could be caused to shine on the acetate. The electrifications of air were the same in kind as those already described, and were of about the same amount as those obtained with the nitrate of uranium. When the ultra violet light was shone on the acetate the electrification of the air produced did not differ from that which was produced with ordinary light.

Note by Lord Kelvin on the sign of the electrification found in air drawn from space surrounding electrified uranium.

In some of our previous experiments with high voltages we found sparks to pass between uranium and other metals* apparently according to the laws of disruptive discharge subject to but little modification by the special quasi-conductivity induced in air by the 'uranium rays.' On the other hand, all our experiments with voltages, less than 500 or 600 volts per cm. of line of force in the air at ordinary atmospheric pressure seem to be not sensibly influenced by disruptive charges or by brushes; and the quasi-conductivity of air induced by uranium was the dominant factor. This is undoubtedly the case in the experiments now described by Dr Beattie, and I assume it to be so in what follows, except when I give express warning of possible liability to disruptive discharges.

The effective conductivity induced in the air by the uranium influence is of course greatest in the immediate neighbourhood of the uranium, but there is something of it throughout the enclosure. Hence it may be expected that electricity of the same kind as that of the uranium will be deposited in the air close around it, and electricity of the opposite kind in the air near the enclosing metal

* Kelvin, Beattie, and Smolan, *Proc. R.S.E.*, 1897.

surface. According to our former experiments * the quantity flowing from either the uranium or from the surrounding metal per sq. cm. of its surface increases but little with increased voltage when this exceeds 5 or 10 volts per cm. Now, when the greatest diameter of the uranium is small in comparison with distances to the outer metal surface, the voltage per cm. is much greater along the lines of force near the uranium surface than near their outer ends on the surrounding metal. Hence the rate of discharge of electricity into the air from the uranium will cease to increase sensibly with the difference of potential between the uranium and the surrounding metal, while the rate of discharge of the opposite electricity from the large surrounding metal surface is still notably increasing. Hence if the dimensions and shapes of the uranium and of the surrounding metallic surface are such that for small voltages, such as 10 or 20 volts of difference between the uranium and the surrounding metal, the electricity lodged in the air by discharge from the uranium preponderates over that discharged from the surrounding metal, the excess must come to a maximum and diminish, possibly even down to zero, with greater differences of potential: and at potential differences still greater the electricity lodged in the air from the outer metal may preponderate, and the electricity in the air drawn off and given to the filter be of opposite sign to that of the uranium which was found with the lower voltages: *provided the configurations are such, and the voltages are so moderate that disruptive discharge does not intervene to any practically disturbing extent.*

* Kelvin, Beattie, and Smolan, *Proc. R.S.E.*, 1897.

On Supersaturation and its Dependence on Crystalline Form. By W. W. J. Nicol, M.A., D.Sc., F.R.S.E., F.I.C.

(Read May 3, 1897.)

The phenomenon of Supersaturation in certain well-marked instances is familiar to every student, and may briefly be defined as consisting in the retention in solution at a low temperature of a quantity of a substance sufficient to form a saturated solution at a higher temperature. So long as the temperature is not allowed to fall below a certain point, and the solution is not brought in contact with a particle of the solid substance or with a substance isomorphous with it, the solution is in perfectly stable equilibrium. The researches of Gernez and many others have established this now generally accepted fact, that, the temperature remaining constant, the sole cause which is able to disturb the equilibrium of the solution is contact with a crystal of the same form as that which will crystallise out from the solution.

In a paper communicated to this Society nearly twelve years ago,* I described a series of experiments on supersaturation which had forced me to the conclusion that in the strict sense of the word no solution is ever supersaturated, a conclusion which I found had been reached by Loewel † nearly fifty years before, as the result of his investigations on the solubility of sodium sulphate. Shortly stated, the views of Loewel and myself were that a supersaturated solution is a saturated or non-saturated solution of the anhydrous salt, the term anhydrous being used by me as a convenient means of indicating that no definite hydrate exists in the solution, but that the whole of the water is in the same relation to the salt.

In the paper referred to above, and in two ‡ subsequent ones, I showed that a supersaturated solution was able to dissolve more salt even when it was as concentrated as the solution obtained by

* *Phil. Mag.*, June 1885, p. 453.

† *Ann. d. Chim. et Phys.*, (3), xlix. p. 51.

‡ *Phil. Mag.*, September 1885, p. 295; *Journn. Chem. Soc.*, 1887, p. 389.

using dry crystals of sodium thiosulphate ($Na_2S_2O_3,5H_2O$); also, that supersaturated solutions can be easily prepared in the cold, and therefore that supersaturation is not a property conferred on the solution by heat, as many supposed; and, further, that, by evaporation at the ordinary temperature, supersaturated solutions yielded crystals, not of the ordinary hydrate, but of a lower one containing less water. Other experiments—for an account of which reference must be made to the original papers—showed conclusively that the physical properties of a solution underwent no abrupt change when it passed into the region of supersaturation, either by fall of temperature or by increase in its concentration.

The question whether supersaturation ever occurs in the solutions of salts which crystallise without water is generally regarded as undecided, while the many well-marked examples to be found amongst organic compounds are passed over in silence, inasmuch as the conditions preclude the possibility of the formation of hydrates or compounds analogous to them, and consequently an explanation which is fairly satisfactory in the case of hydrated salts is here inapplicable.

Shortly after the publication of the experiments on supersaturation referred to above, my attention was drawn to the work of Frankenheim * on the crystalline form of potassium nitrate. I had found in the course of other work that this salt was abnormally soluble in solutions of sodium nitrate, and it struck me that this was probably due to the solubility of the unusual, hexagonal modification of potassium nitrate isomorphous with sodium nitrate being greater than that of the usual rhombic modification. I therefore repeated Frankenheim's experiment: a small drop of strong solution of potassium nitrate was placed under a low-power microscope, and allowed to evaporate spontaneously at the ordinary temperature; as evaporation proceeded, rhombohedral crystals made their appearance, and in some cases continued to grow in size and number until the water had entirely evaporated. In other cases, more usually, rhombic prisms put in an appearance after a time, growing with great rapidity, thus indicating supersaturation. It was further found that if a drop from which a

* *Pogg. Ann.*, 1837, 40, p. 447; 1854, 92, p. 354.

number of rhombohedra had crystallised out, which was therefore undoubtedly saturated to that form, was stirred with a needle which had been cleaned by heating to redness, no crystallisation of the rhombic form was induced, but if the needle was touched with a rhombic crystal and again brought in contact with the solution, instant separation of the rhombic variety ensued, and the rhombohedra, when touched by the prismatic crystals, became opaque and developed prisms shooting out on all sides. In short, the solution exhibited all the phenomena of supersaturation.

This may be taken as the crucial experiment, the one which gives us the key to all examples of supersaturation, enabling us to state the proximate cause of all such cases, as well as those of superfusion, as follows:—*Whenever under the conditions of experiment two allotropic modifications of the dissolved or fused substance can exist, then supersaturation or superfusion, as the case may be, is also possible.* The term allotropic is here used in a somewhat wider sense than usual, as including not only amorphous and different crystalline forms of the same substance but also different crystalline forms due to the presence or absence of foreign molecules, such as water of crystallisation. The above is the fundamental condition which must be fulfilled if supersaturation is to occur at all. Two minor conditions limit the range within which such supersaturation can exist: these are concentration and temperature of the solution, and are interdependent, though it does not follow that we are able to attain either limit. Thus, in the case of temperature, the lower limit may lie below the so-called cryohydrate point, where the conditions are altered by the existence of new phases; but for every concentration there is a critical temperature, and for every temperature a critical concentration, outside the limits of which the supersaturation breaks down, not by the separation of the less stable form but the formation of the usual stable modification.

It is not my intention in this communication to enter fully into the experimental evidence on which the foregoing conclusions are based. I had intended to delay the publication until I had accumulated a mass of facts sufficient to place the conclusions derived from them beyond the possibility of doubt, but the appearance of a paper by Ostwald in the *Zeitschrift für physikalische*

Chemie, published a few days ago, has compelled me to choose between the disadvantages attending too early publication, and the risk of finding my results published by another and independent observer. I shall therefore content myself with placing on record as succinctly as possible the leading experimental results on which the foregoing conclusions are based, reserving for a future communication the details and the discussion of them.

1. Supersaturation caused by allotropy due to the presence of foreign molecules, generally water of crystallisation.

(*a*.) A supersaturated solution of sodium thiosulphate evaporated at the ordinary temperature deposited crystals containing 15 to 20 per cent. of water, indicating the presence of a hydrate containing not more than $2H_2O$. All results of this nature obtained with supersaturated solutions are necessarily approximate and are always too high, as it is impossible to drain off the mother liquor or to dry the crystals. Under the microscope it is easy to distinguish the crystals of the one hydrate from those of the other, and on warming the pentahydrate dissolves before the other.

(*b*.) A supersaturated solution of sodium sulphate deposits crystals of the heptahydrate, or of the anhydrous salt, according to the temperature, the former below, the latter above 20° C. The former was observed by Loewel, but, so far as I am aware, the latter had been obtained only by heating the decahydrate.

(*c*.) A supersaturated solution of borax evaporated at the ordinary temperature deposited crystals containing five molecules of water, as already observed by Gernez. Under the microscope the two are easily distinguished, and the decahydrate dissolves much more easily on heating than the pentahydrate.

(*d*.) A warm saturated solution of barium chloride deposits under the microscope quadratic crystals, which become surrounded by eight-sided plates, the rectangular plates becoming rounded and finally disappearing, leaving, however, a mark on the new crystal roughly corresponding to the outline of the original crystal. This has been also observed by Lehmann, who states that the first crystals probably consist of a lower hydrate. A supersaturated solution of barium chloride can be prepared only with the greatest difficulty, and is extremely unstable.

(*e*.) Lead acetate: the limit of temperature for concentrated

solutions of this salt is 35° C. The fused crystals when evaporated in a thermostat at 40° C. deposited crystals containing, at the most, one molecule of water.

2. Supersaturation due to the existence of allotropic forms, whether enantiotropic or monotropic.

(*a.*) Potassium nitrate, already described.

(*b.*) Ammonium nitrate, fully described by Lehmann, forms supersaturated solutions.

(*c.*) Silver nitrate, hitherto known only in the rhombic modification, deposits hexagonal rhombohedra (?) when a drop is allowed to evaporate spontaneously; and when the drop saturated to this modification is touched with a needle point which has been in contact with the rhombic variety, instant crystallisation of the whole ensues, showing supersaturation. Supersaturated solutions on a larger scale can also be prepared by the exercise of some care.

Organic Compounds.

(*d.*) Acetanilid. A hot concentrated solution deposits, at different parts of the microscope field, rhombic ordinary crystals and (rhombic ?) crystals of a modification. These, when in the immediate neighbourhood of the stable form, are gradually dissolved; but if the two come in actual contact they become opaque, owing to change of their internal structure. Supersaturated solutions on the large scale can be readily prepared.

(*e.*) Hydroquinone. A hot strong solution deposits a net-work of crystals of an unstable modification (hexagonal ?). After a time monoclinic prisms of the ordinary modification put in an appearance, and gradually eat their way through the unstable form. Supersaturated solutions can be easily prepared.

(*f.*) Acetamide. A concentrated solution deposits crystals prismatic in shape of an unstable form. When the edge of the drop is touched with a trace of the ordinary hexagonal form, the whole crystallises, the original crystals becoming opaque. Supersaturation on the large scale is very marked.

(*g.*) Malonic acid. This substance forms supersaturated solutions very readily. When a hot strong solution is observed under the microscope, prismatic crystals of the unstable modification soon

cover the field, and these after a time become changed in places; triclinic crystals of the ordinary modification make their appearance, which slowly eat away the unstable modification.

(*h.*) Mandelic acid. Supersaturated solutions can be easily prepared on the large scale. A hot concentrated solution deposits a feathery crystalline modification, succeeded by the rhombic plates of the stable form, which grow slowly at the expense of the unstable form.

(*i.*) Resorcin; readily forms supersaturated solutions in quantity. A strong hot solution deposits prismatic crystals of the unstable form, which sometimes remain unchanged for a considerable time, but ultimately crystals of the ordinary rhombic variety make their appearance, and grow rapidly at the expense of the other.

(*j.*) Tartaric acid. The limits of supersaturation appear to be very narrow, but a very concentrated solution gives a mass of crystals of a new modification, followed later on by the ordinary monoclinic variety, which slowly eat away the crystals first formed.

(*k.*) Citric acid. This forms supersaturated solutions with great ease, and, inasmuch as the ordinary crystals contain one molecule of water, it ought to have been included in class 1; but the supersaturation is only partly due to water of crystallisation, for a long and careful study of the behaviour of a solution crystallising under the microscope has satisfied me of the existence of no less than four crystalline citric acids. Of these four forms, one, the ordinary rhombic modification, contains of course one molecule of water, and is the ultimate stable form; the others are either anhydrous or contain less water than the former, and are possessed of different degrees of stability. As the solution cools, the first crystals to appear are prismatic in form, succeeded or accompanied by others consisting of twinned plates arranged about a common centre, which slowly eat away the first formed crystals. At the same time, the first formed crystals are seen to undergo a change, becoming opaque, and where the edges have been rounded by the action of the plate variety, fresh prisms shoot out, never, however, reaching the latter, but after a time are, in their turn, eaten away by it Finally, if the edge of the drop be touched with a crystal

of the stable form, a change creeps over the whole of the prisms, which begin to grow again at the edges, and ultimately eat up the plate variety.

Finally, I am indebted to Ostwald's paper for yet another example under this head, but among inorganic compounds,—sodium chlorate. This salt, he states, forms supersaturated solutions, and a drop evaporating under the microscope is seen to deposit rhombic plates, which are succeeded by cubes, which slowly eat up the former crystals. Ostwald assumes that the first formed crystals are hydrated: had he realised that this is not the case, he would, I believe, have arrived at the explanation of supersaturation I have given above.

The point here raised by Ostwald requires examination:—Is there any reason to suspect that the allotropic forms I have observed in the case of compounds usually crystallising without water consist of hydrates? I have no hesitation in answering this question in the negative, for the following reasons:—All the unstable modifications of hydrated salts have been found to contain less water than the stable forms, and further, several of the modifications described by me as occurring with supersaturated solutions have been obtained by Lehmann by simple fusion of the dry substance; and finally, the very examples cited by Ostwald in support of his assumption that the modification of sodium chlorate is hydrated are arguments against it, for the hydrated forms of sodium chloride and bromide are formed only at low temperatures, and follow, not precede, the separation of the anhydrous form as the temperature falls, that is, as the solution becomes more concentrated; and the observation of Löwig, which requires confirmation, that sodium bromate crystallises at $-4°$ C. with one molecule of water, is also an argument against the probable existence of a hydrate of the chlorate when it is remembered that no less than five hydrates of the iodate are said to exist.

In the foregoing, mention has been made of different crystalline forms only, but it is clear that what has been said applies equally to crystalline and amorphous forms. In cases where supersaturation is caused by the existence of an unstable amorphous form, and the temperature is above the critical point, it is most probable that there will be infinite solubility, which leads us at once to super-

fusion, subject to the same laws as supersaturation, and differing from it only in the absence of a solvent.

In very many instances it may not be possible to obtain the unstable form, whether it be crystalline or amorphous; but if it is found on further investigation that supersaturation is possible in *all* cases where *two* forms can be obtained, I would submit that this will equally establish the truth of the law laid down in the early part of this paper, and it is in this direction that I propose to continue the work, at the same time endeavouring to ascertain the limits within which supersaturation is possible, and the examination of the border region within which the supersaturation or superfusion is terminable by shock or other mechanical means. That such a region exists is, I believe, indicated by many experiments already made, and its existence would explain the many conflicting statements made by previous observers as to the causes which bring about the crystallisation of supersaturated solutions.

Note on the Calcutta Earthquake (June 12, 1897), as recorded by the Bifilar Pendulum at the Edinburgh Royal Observatory. By Thomas Heath, B.A. (With Two Plates)

(Read July 5, 1897.)

On the 12th of June a disastrous earthquake occurred in North-Eastern India. The violence of the shock was first felt in Calcutta (at least, the first report of it reached us from that quarter), and spread with destructive violence, north-easterly through Assam along the valley of the Brahmapootra, and north-westerly along the course of the Ganges and the base of the Himalaya Mountains. We may form some idea of the violence of the shock from the disastrous effects produced by it. From Calcutta it is reported that few houses have escaped without damage of some description, that part of the Cathedral spire has fallen, and that many public buildings have been injured. At Darjeeling many houses were destroyed, and the district left without railway communication. In Bengal the destruction of property was apparently not so great; but in Assam the shock is reported to have spread ruin far and wide, and to have been attended with serious loss of human life.

The destructive energy of the earthquake appears to have been exhausted within the area thus briefly indicated. The undulatory movements set up by it in the earth's crust have, however, been detected at great distances from the centre of the disturbance, and already from three stations in Western Europe reports are to hand showing that seismographic apparatus have recorded the phenomenon beyond any possibility of doubt. These stations are Grenoble in South-Eastern France, Prof. Milne's Seismological Observatory in the Isle of Wight, and the Royal Observatory on Blackford Hill.* Doubtless in time we shall hear of similar records obtained at many other places.

I trust it will interest the members of the Royal Society if I

* Professor Tacchini announces that the earthquake was also recorded at Rome.

call their attention for a few moments to the instrument with which this observation has been recorded at Blackford Hill,—the bifilar pendulum, as it is called. I do not intend to enter upon a detailed description of the instrument, because it has already been described in the Reports of the British Association for the Advancement of Science, and elsewhere. I may remind the members of the Society, however, that the essential feature of the instrument consists in the reflection of a ray of light from the surface of a concave mirror, suspended in a loop of fine silver wire from two points nearly vertically over one another. If the upper point of support be moved slightly in a direction perpendicular to the plane of the wire, the mirror will rotate around its vertical diameter, and the amount of the rotation, or the sensitiveness of the instrument for any given displacement of the upper point, depends on the closeness of that point to the vertical line passing through the lower one.

At the Royal Observatory the instrument and its recording apparatus are so placed that the plane of the wire lies east and west when the reflected ray falls on the middle of the roll of photographic paper. The mirror is therefore set in motion by displacement of the upper support in a north and south direction, or by the north and south component of displacements in any other direction, and movements in the east and west direction do not affect it, except to alter its sensitiveness. With the object of securing that the mirror shall not be disturbed by the slight vibrations of short duration set up in the rock by concussions happening in its immediate vicinity, it is immersed in paraffin oil of the most transparent kind obtainable. It is thus sensitive only to tremors or undulatory movements of the rock of some duration. That this is so has, I think, been amply shown by observation. I have never, so far, been able to find on the photographs any effect arising from such causes as slamming of doors, or the fall of heavy weights within the building, or the explosions which are constantly taking place at a quarry on the south of the hill, though I have noted the times of many of these explosions, and examined the photographic record for the purpose of determining whether any effects were produced by them. On the other hand, observations made by Dr Copeland and myself at Calton Hill showed that the

weight of a man standing on the rock, about 15 inches from the centre of the instrument, at a point north or south of the plane of the wire, caused the mirror to rotate towards the side at which the weight was placed.

The instrument was fixed in position at Blackford Hill early in August of last year, and we have thus, so far, eleven months of its photographic record. Before this it was mounted at the Calton Hill Observatory from March 1894 to October 1895, but during this period we had no photographic recording apparatus, and the observations were made once a day by eye, and, of course, only those disturbances, naturally few in number, which happened at the time of observation were detected. The experience gained, however, in the working of so sensitive an instrument as this has not been without its use, as I believe it has enabled us to obtain a more continuous record at Blackford Hill than we would otherwise have secured.

An examination of the photographic record for the eleven months just referred to shows at once that the disturbances which affect the instrument may be divided into at least three characteristic groups.

In the first group I would place all those disturbances which show a widening and blurring of the photographic trace. This is the least numerous class, and the best marked examples of it show gaps in which there is no trace of effect on the paper. They are evidently caused by successive oscillations of the mirror; and when these are of great amplitude, the exposure to light at some points does not appear to be sufficiently long to produce photographic result.

The first of these was found on the photograph of August 26, 1896, which has two of them, and there is another on the record of Sept. 5. All three must, without doubt, be referred to the violent shocks of earthquake which were experienced in Iceland on these dates. The two disturbances of Aug. 26 were also observed in Paris. The next well-marked examples of these gaps occur on the 19th Feb. 1897, when there are again two of them, with an interval of about three hours between them, both of which were observed by Prof. Tacchini at Rome, and one of them by Prof. Milne in the Isle of Wight. The most remarkable record of all we

have yet secured, however, appears on the photographic trace of the 12th of June (figs. 1 and 2), the date of the great earthquake at Calcutta. Special interest attaches to the record of this disturbance, because of its long duration and great amplitude, and also because of the distinctness with which several of the separate oscillations of the mirror appear on the paper. The duration of the earthquake at Calcutta is stated to have been, according to one account, only "some seconds," according to another "fully five minutes." However this may be, the undulatory movements arising from it cover a period at Blackford Hill of nearly two hours. Very slight preliminary tremors commenced at 11.18 in the forenoon and lasted ten minutes. The more violent oscillations commenced at 11.28 and lasted up to 0^h 33^m, thus covering a period of 1 hour 5 minutes, and after this lighter tremors can be traced up to 1.12. The amplitude, when at its maximum in the distinctly photographed part of the record, I found to be equivalent to an angular movement of the frame which supports the wire of about 20 seconds of arc, and we are justified in assuming that in the unphotographed part, or gap, the amplitude must have been still larger.

The first newspaper reports of the earthquake gave 5 o'clock P.M. as the time at which the shock was felt at Calcutta, and I have not since seen any more precise statement of the time of its occurrence, —5 P.M. at Calcutta corresponds to 11.6 of the forenoon in Greenwich mean time. The first oscillation of large amplitude occurred here at 11.28. If we put aside the lighter tremors, and suppose for the moment that the first large oscillation shown here was the result of the first violent shock at Calcutta, we have twenty-two minutes as the time taken by the seismic wave to travel through the earth's crust from Calcutta to Edinburgh, a distance in round numbers of 4970 miles on a great circle, or 226 miles per minute.

At Grenoble the record of this earthquake was registered on the seismograph at 11.28. If I am right in supposing this to be Paris mean time, it would correspond to 11.19 Greenwich mean time, or one minute later than the beginning of the preliminary tremors at Blackford Hill.

It may be interesting to note that, with the exception of two very slight tremors on the afternoon of the 11th, there is not a vestige of any kind of disturbance on the photographs preceding

that of the 12th till we go back as far as the 3rd June. Thus for nearly eight days the mirror was perfectly quiescent. On the other hand, between the remarkable disturbance of the 12th and the noon of the 16th there are four well-marked tremors to be seen, probably due to readjustment of the strata following the great shock of the 12th.

I have now called your attention to all the more interesting examples of the first group of records. The second group or class comprises all those records which appear to have been produced by a single sudden tilt of the frame of the instrument, causing the mirror to rotate about its vertical diameter in one direction only, without any apparent trace of rapid oscillation in the opposite direction. The mirror returns to its normal position after some hours, but so slowly that this movement is quite masked by being mixed up with the slight swerving of the mirror which frequently takes place, and is due probably to seasonal changes of temperature. Sometimes three or four of these tilts are found following one another in the same direction at intervals of several hours. More often they occur in opposite directions alternately, with intervals varying from several hours to five or ten minutes between them. In the case of the short intervals, they have an appearance on the curve like the tooth of a saw.

An interesting example of three tilts in the same direction is found on the record of Feb. 6 and 7 (fig. 3). The first occurred at $19^h\ 33^m$ on the 6th, a second at $5^h\ 35^m$ on the 7th, and the third at $13^h\ 20^m$. They are each towards the north, and are followed by a perfectly straight line sloping towards the south. Probably the sloping position of the lines is caused by the combined effect of the return movement of the mirror and the temperature swerving. On the same day records were found on the instruments at Rome and the Isle of Wight, but with this class of disturbance the identification of the records at different stations is not so satisfactory as it is in the case of the first group. The photograph of Jan. 28 (fig. 4) shows four sudden tilts towards the north, at shorter intervals than those of Feb. 6 and 7. Each tilt is again followed by a line sloping towards the south. There is no evidence, that I have seen, so far, of the disturbances in this case having been recorded elsewhere.

The photograph of March 8 (fig. 5) shows a curious series of four sudden tilts to south, each followed by an equally sudden tilt to north, forming four of the saw-teeth I have referred to. Two days later there is a second set of three, very similar in character.

In a third group of these records we might place a large number of irregular bends of the curve, which differ from the second group in being more gradual in their character. There is no appearance of any sudden effect having been produced on the mirror.

The photograph of Oct. 20 (fig. 6) shows such a number of these irregular bends as to be almost bewildering. I find it stated in the Monthly Bulletin of the Geodynamic Department of the Observatory at Athens that a "feeble shock" was experienced in part of Greece on this date. It is greatly to be regretted that the valuable assistance rendered to seismological research by the issue of this monthly publication is no longer to be reckoned upon. Notice has been received at the Observatory within the last few days that the publication of the list of earthquake records observed in Greece is to be discontinued,—as a result, I presume, of the unfortunate political troubles in which that country has been plunged.

One more photograph (fig. 7), the last I have to call your attention to, shows a number of curious bends, but whether they have any connection with a severe shock of earthquake which I find reported as having occurred in South Australia and Western Victoria on May 10, the date of the photograph, I am quite unable to say.

The question may very naturally be asked—I might even say, the question has been asked—What place has such an instrument as this in an astronomical observatory? The Observatory at Blackford Hill has been established for the purpose of astronomical research as its first and most important object, but it is also intended that physical researches having any bearing on astronomical questions may also be taken up as opportunities for doing so present themselves; and it is hoped that we shall be able to show hereafter that the work we have commenced with the bifilar pendulum has a very distinct connection with the meridian work of an astronomical observatory. In observing the declinations of stars, the fundamental index-point to which all measures are

referred is, of course, the nadir reading of the circle of the instrument. In practice this reading is made every observing night at the commencement of the night's work, and again at its conclusion. If the night's work is a long one, an intermediate reading should also be made. It has frequently been found that two consecutive readings differed from one another by an amount which could not be accounted for either by temperature change in the instrument or by errors of observation, and it has hitherto been found impossible to refer these anomalous readings to any known cause. For instance, in looking over a series of observations of known stars made by myself some years ago with the mural circle at the Calton Hill, for the purpose of re-determining the latitude of that Observatory, I find there are two nights in which the nadir readings differed by six-tenths and seven-tenths of a second of arc respectively. The principal object Prof. Copeland had in view in establishing the bifilar pendulum at Blackford Hill was not so much the study of earthquakes and the phenomena connected therewith, as the possibility of showing that the anomalous readings of the nadir point were caused by, or at least had some connection with, changes in the direction of the vertical of the transit circle, resulting from undulatory movements in the earth's crust such as this pendulum seems admirably adapted to show. In the case of my own observations just referred to, the latitude results from each star would come into closer agreement if, supposing an earth tremor to have taken place, we divide the observations into sets and refer them to the separate nadir readings, instead of using the mean of these readings for all stars alike, according to the usual practice. But the pendulum was not then established, and, besides, my observations were not sufficiently numerous to draw any inference of this kind from them. As yet, we have had no opportunity at Blackford Hill of putting this question to a practical test.

Outside the range of astronomy, however, and in a department of science which the Society of Psychical Research claims as exclusively its own, there would seem to be a possibility that instruments very similar to the bifilar pendulum in principle, though different in design, might be made use of with considerable hope of success. The interesting, if not also amusing, correspond-

ence which has lately been going the rounds of the public press, under the heading "On the Trail of a Ghost," has suggested to no less an authority on seismological science than Prof. Milne, that the mysterious noises heard might very well be caused by earth tremors. If this be so, to trace the connection between cause and effect, and "lay the ghost," would seem, as he suggests, to be by no means difficult. It is surely a pity that the attempt should not be made, as the case appears to be one in which physical science might well be expected to render some assistance.

Fig. 1 is reproduced from the bifilar pendulum photographic record of the Calcutta earthquake, enlarged to twice the original size.

Figs. 2 to 7 inclusive are reproduced from photographs of various dates, reduced to $\frac{2}{5}$ of the original size, and embrace from 36 to 48 hours each. The straight line accompanying each of the pendulum curves is photographed by a fixed mirror, and serves as a base and time line. The disturbances marked 2″ are measures of the sensitiveness of the instrument.

MR. THOMAS HEATH ON THE CALCUTTA EARTHQUAKE (JUNE 12, 1897).

FIG. 1.

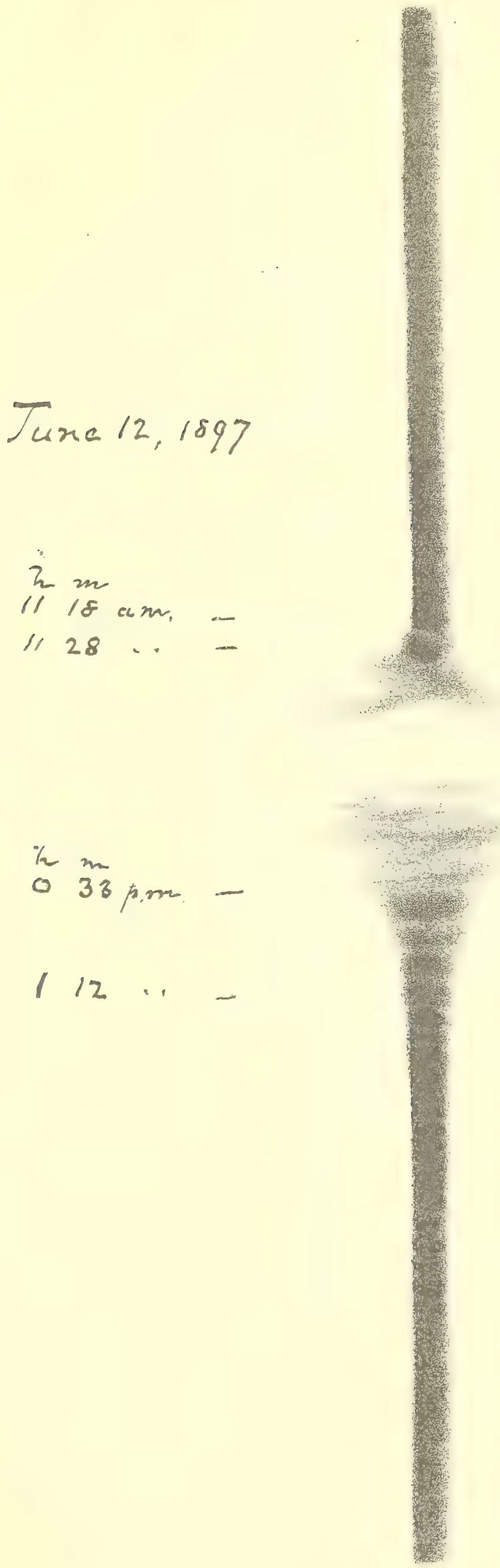


Fig. 6.

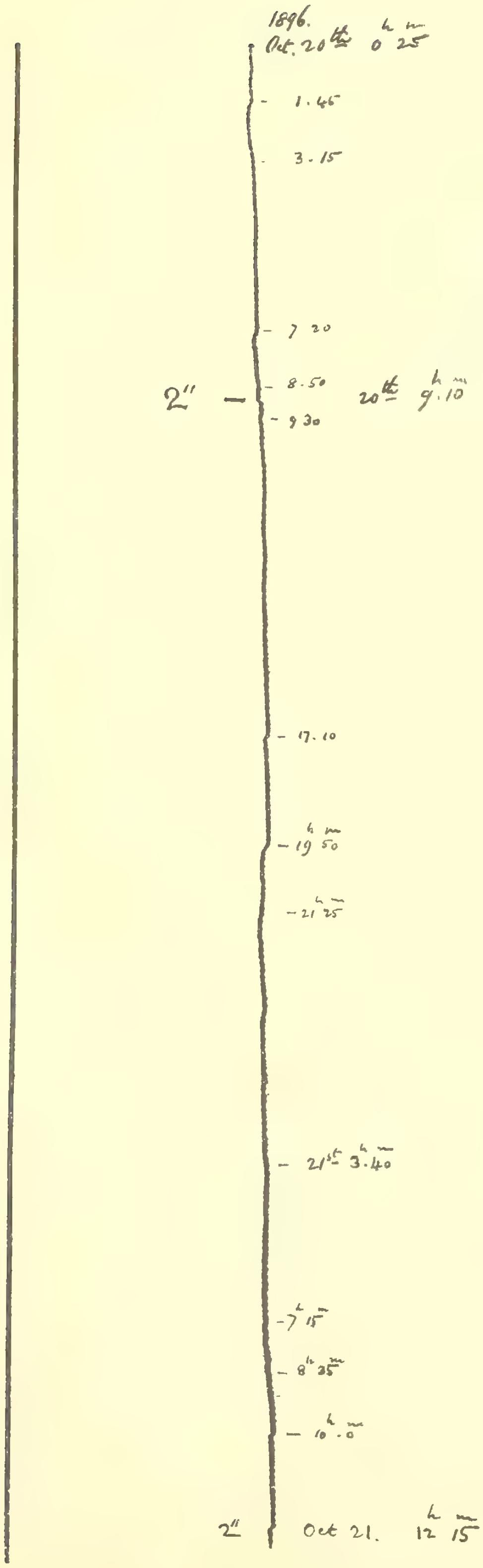

1896.
Oct. 20th 0h 25m
1.45
3.15
7 20
2″
8.50
20th 9h.10
9 30
17.10
19h 50m
21h 25m
21st 3h.40m
7h 15
8h 25m
10h.0m
2″
Oct 21. 12h 15m

Fig. 7.

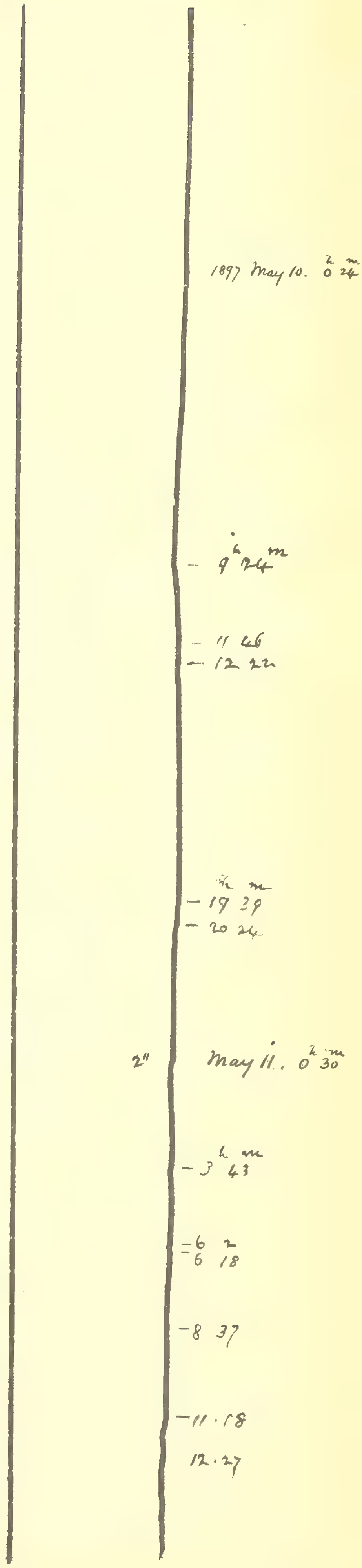

1897 May 10. 0h 24m
9h 24m
11 46
12 22
19h 39m
20 24
2″
May 11. 0h 30m
3h 43m
6 2
6 18
8 37
11.18
12.27

Fig 2

Fig 3

Fig 4

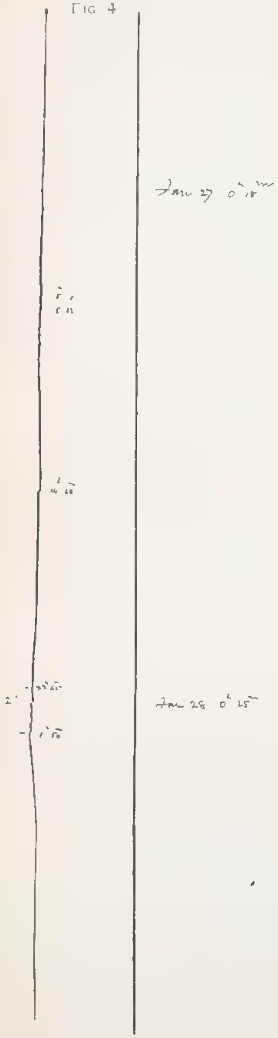


Fig 5

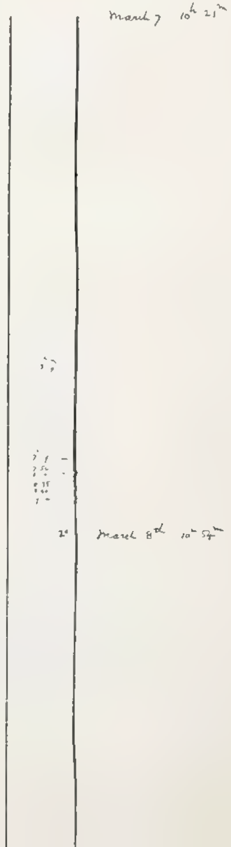


Fig 6

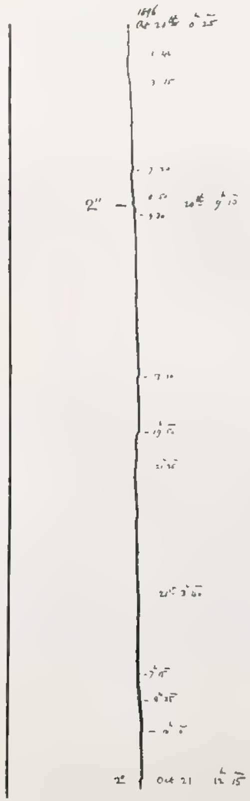
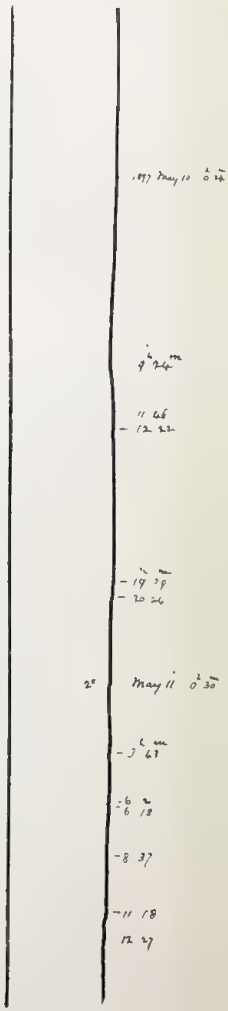


Fig 7



Notes on the Total Eclipse of the Sun, 8th August 1896. By the Astronomer-Royal for Scotland and Mr A. J. Ramsay.

(Read July 19, 1897.)

In the Total Eclipse of 8th August last year, the line of totality began at a point to the north of the Shetlands, passed across the north of Norway, from Bodö on the west to Vadsö in the north-east, then traversed Novaya Zemlya, Siberia, and Yesso, the northern island of Japan, ending finally in the North Pacific Ocean. Both Bodö and Vadsö are easily accessible by the Norwegian mail-boats, the former being, however, some four days nearer than Vadsö. At the first glance one would naturally inquire:—Why was not Bodö chosen for our station in preference to the more distant Vadsö? The chief reason was the very important one that at Vadsö the sun, at the time of total eclipse, attained an altitude of 15° above the horizon, while at Bodö the altitude was only some 7°; at Vadsö, too, the duration of totality was 106 seconds, as compared with 101 seconds at Bodö. As regards the chances of a clear sky, there was not much to choose between the two stations; on the west coast the rainfall was reported to be relatively large, while fogs were said to be of frequent occurrence at Vadsö. Vardö, some 40 miles east of Vadsö, was declared unsuitable on account of a still greater likelihood of trouble from sea fogs in the early morning, during summer. We therefore fixed on Vadsö as our observing station, and communicated with Professor Mohn of Christiania, the well-known meteorologist, as to the best steps to be taken for securing a favourable site for our instruments. We are indebted to him for much valuable advice, and to Dr Caspersen of Vadsö for his very material help in securing comfortable rooms for us there.

The chief lines of observation to be adopted by an eclipse expedition may be summed up as follows:—

1. Visual telescopic observations of the times of contact of the moon's limb with that of the sun.

2. Visual telescopic observations of the corona and prominences.
3. Visual spectroscopic observations of the chromosphere, and polariscope observations of the corona.
4. Direct photographic records of the corona and prominences.
5. Photographic records of the chromosphere and other solar envelopes.

As the photographic methods are at present the most likely to add to our knowledge of the sun's constitution, we decided to direct all our efforts to the fitting out of a photographic equipment which should include apparatus of as efficient and varied a character as possible. This could be effected with little difficulty, as the Edinburgh Royal Observatory possesses a variety of spectroscopes and cameras well suited for an eclipse expedition. The instruments we took with us comprised:—

1. A forty-foot telescope with Dallmeyer photographic lens of 4 inches aperture.
2. A 6-inch equatorial with object-glass prism.
3. A camera with a quartz lens of 1·8 inch aperture and Iceland-spar prism, mounted on the same stand as the 6-inch equatorial.
4. A camera of 4 inches aperture and 33 inches focal length, equatorially mounted.
5. A whole-plate camera by Dallmeyer with a lens of $1\frac{1}{4}$ inch aperture and 10 inch focus.

These instruments we shall describe a little more fully.

1. 40-*foot telescope.*—The object in employing a telescope of great focal length is to obtain large direct negatives of the sun, which require no further enlargement. The diameter of the sun's image in this instrument is $4\frac{1}{10}$ inches. But since it is impracticable, on an expedition at least, to mount a tube of this length equatorially, the difficulty of following the sun's diurnal motion was overcome by fixing the tube on wooden trestles so as to point exactly to the place in the heavens which the sun would occupy at mid-eclipse, the photographic plate being mounted on a slide which moved in the proper direction and at the speed required to keep the sun's image central. This plan had already been employed with great success by Prof. Schaeberle, of the Lick Observatory, in Chili, in 1893, but our instrument was the first of the kind tried in Europe.

The tube was made of thin galvanised iron in 8-foot lengths, suitably tapered to facilitate packing and fitting together, the lower end being 26 inches in diameter, the upper 12 inches. The lens was mounted on a metal carrier securely fixed on the top of a substantial wooden tripod, and was adjusted by means of a delicate centring apparatus. In order to prevent possible vibration of the tube from disturbing the lens, it was connected with the tube by a collar of black cloth only. The exposing shutter at the upper end of the tube was opened by a cord passing into the hut in which the observer was stationed; on slackening the cord, the shutter closed of itself. At the lower end of the tube a small dark room was built of strong brown paper covered with waterproof material. In this dark room was mounted the slide for the photographic plates, which is here shown. The angle between the plane of the plate and the vertical is, of course, the altitude of the sun at totality, while the inclination of the slide to the horizon in that plane is the parallactic angle plus a small correction for the sun's motion in declination and the change of refraction. The driving-clock, which was kindly lent by Lord M'Laren, is so arranged as to move the plate at a uniform speed of 1·913 inch per minute, corresponding to the apparent angular motion of the sun. The operator stationed in the dark room had sufficient space at his disposal to allow of his putting the plates in position during the eclipse, and was at the same time able to see the sun's image on the sensitive film. The plates used were 18 inches square, so as to give a picture of a large part of the corona. By arranging four lead pencils carried by levers, we were able to use the slide motion as a chronograph for recording the approximate exposures made by all the observers.

2. *The* 6-*inch Simms telescope*, with its large prism in front of the object-glass, was mounted on a heavy Cooke equatorial stand with driving-clock. The advantage of this form of instrument is that, in the case of a stellar spectrum, the source of light being a point at infinite distance, no slit or collimator is required; while in the case of the eclipsed sun the ring of chromosphere acts like a narrow slit of circular shape, and we obtain a series of overlapping rings corresponding to the various refrangibilities of the light given out by the chromosphere. Provision was made for nine exposures with this telescope.

3. *The Iceland-spar camera* had a quartz objective of 1·8 inch aperture, achromatised for the violet rays by a meniscus of Iceland-spar. As this instrument was designed for photographing the ultra-violet spectrum of the chromosphere and prominences, the use of glass (so highly absorbent of ultra-violet light) was inadmissible. The prism was therefore made of Iceland-spar, the material for which we fortunately had at the Observatory. Prof. Tait kindly lent us the quartz lens, and also provided the spar from which the compensating lens was cut. The prism and compensating lens were figured by Bernard Halle of Steglitz, who is specially skilled in working the more intractable crystalline substances. Our full programme included nine pictures with this camera. It was conveniently mounted as a partial counterpoise to the 6-inch equatorial.

4. *The Dallmeyer eclipse camera* of 4 inches aperture and 33 inches focus was intended for photographing the sun's direct image. The lens, which is of unsurpassed quality, was most successfully used by Mr Henry Davis at Baikul, Southern India, in 1871, on behalf of Lord Crawford. This camera was mounted on a separate equatorial stand provided with driving-clock and finder. Provision was made for fifteen exposures of various lengths.

5. *The whole-plate camera* of $1\frac{1}{4}$ inch aperture was attached to the 6-inch stand, and it was intended that one plate should be exposed for the whole time of totality; in this way, though the plate would be over-exposed for the inner corona, we hoped to obtain a picture of the fainter outlying coronal streamers. The angle covered by the lens is 44 degrees.

All the instruments were mounted at Blackford Hill and carefully tested in the beginning of July, so that everything was in complete order by the 11th, when they were sent off in advance of the party. The members of the expedition were Mr A. J. Ramsay, Assistant Astronomer; Engineer M'Pherson, our invaluable factotum; my son Theodore (who accompanied us as a volunteer), and myself.

On July 18 we sailed from Newcastle to Bergen, proceeding thence by the Norwegian mail to Vadsö, where we arrived on the 28th. As we were the first astronomical party to arrive, we had practically the choice of positions, and with the friendly help of

Mr Prebensen, Amtmand of Finmark, we fixed our station on the Fossefjeld, some $2\frac{1}{2}$ kilometres to the north of the town and about 120 metres above sea-level. We were most favourably placed on level ground, sufficiently raised to escape the low fogs on the fjord, should any occur, and quite close to the east end of the northern base line of the Norwegian trigonometrical survey. Our latitude and longitude were thus known with great exactness. From the day of our arrival until the 8th August we were busily engaged in transporting and mounting the instruments; the weather was often wet and trying, but happily by the eve of the eclipse we had everything mounted and satisfactorily adjusted. Owing to prevailing bad weather we had much difficulty in securing astronomical observations for the exact azimuth of the 40-foot telescope; but provided with the Vadsö sheet of the excellent Norwegian survey, we were able at once to place both it and the other instruments approximately in position. The exact adjustments were made later, when the partial twilight of the Arctic summer night enabled us to get some star observations. To make doubly sure that the 40-foot tube was correctly placed, Mr Ramsay computed the time at which the sun should appear in the tube on several mornings preceding the eclipse, and found, to our great satisfaction, that the sun's image appeared at the right time and in the right position on the plate-holder.

In accordance with a scheme already worked out in Edinburgh, each member of the party had a particular instrument allotted to him. Mr Ramsay, as our photographic expert, was in charge of the 40-foot, my son managed the 4-inch camera, M'Pherson looked to the 6-inch prismatic telescope, while I was responsible for the ultra-violet spectroscope and the whole-plate camera. Late on the evening before the eclipse Mr Ramsay filled all the photographic slides, and arranged them in the order in which they were to be exposed, in accordance with the indications of a chronometer and a loud-beating metronome. Early the next morning we were again at our posts with the covers removed from the instruments. Deferring for the present the attempt to describe the general impression produced by the great natural phenomenon, I shall first tell you how we fared with our various cameras.

With a sky almost covered with clouds, one edge of the sun just

peeped out at the time of first contact, but unfortunately it was not the edge on which the moon was at that moment entering. Hence nothing was seen of the first contact. This was the beginning of our troubles. From this moment until the end of the total phase we alternated between hope and fear as we caught occasional glimpses of the sun's diminishing crescent, but barely long enough to verify the setting of the equatorial instruments. As the time of totality drew near the darkness increased rapidly, until at the very last it seemed to fall from the sky, as indeed it really did, the moon's shadow first darkening the air above our heads and then reaching the earth's surface. Forty-five seconds before totality I gave Mr Ramsay the signal to start his driving-clock, but at that time the sky was completely clouded. At the beginning of totality he exposed the first plate, but seeing no image on the moving plate he continued the exposure for 36 seconds, in the hope that the sky might clear. He then tried two other plates, giving 34 and 7 seconds respectively, by which time the total phase was over, without any trace of the sun's image being seen on the film. On the next plate, however, he had the satisfaction of seeing a slender line of light, whereupon he steadily exposed plate after plate until he had secured five pictures of the crescent sun, with exposures of 9, 9, 5, 5, and 9 seconds respectively. [Three of these were shown.] Nothing whatever being seen of the total phase, no exposures were made at that time with the other instruments, except with the whole-plate camera. Immediately afterwards, however, when the thin crescent was seen, exposures were at once made. The negatives taken with the prismatic cameras showed nothing, the clouds near the sun being so bright as to overpower the spectra. Several photographs secured at this time by my son with the eclipse camera serve merely to show the clouds near the sun and to certify that the instrument was correctly focussed. At the last contact the sun was again completely obscured, so that we had not even the consolation of determining the exact time when the moon left the sun's disc.

We knew that all was in readiness and still had a few hours to spare wherein to rest. But our sleep was broken by anxious

thoughts about the prospects of seeing anything at the critical time. About 1 A.M. down came a heavy shower of rain, and matters looked desperate; but by the time we started off for our station at 2.30, the sky had partly cleared. A light westerly wind caused the clouds to drift in such a manner that, if fortune but favoured us, the total phase might eventually be seen through one of the clear patches visible in the north-west. As we trudged up the now familiar pathway carrying our large chronometer with us, it seemed as if all Vadsö was making its way to the heights above the town. A strange excitement pervaded the very atmosphere, and everyone spoke in a lower voice than usual. On reaching our station we found it surrounded by a large crowd composed of widely different elements: there was, of course, the usual type of British tourist; but, besides English, Norwegians, and Finns, there were the Laps, looking picturesque in their gaily-coloured, blanket-like jackets. As the darkness gradually fell over us a hush seemed to pass through the crowd, rendering almost unnecessary the precaution taken by our guard of twenty Norwegian blue-jackets, who kept the people well back, so that we might have no difficulty in hearing the beat of the metronome. Absolutely nothing of the sun was visible as the critical moment approached. At last down came the unmistakable darkness of totality with startling suddenness. For, precisely at the predicted instant, towards us rushed the vast shadow with a speed of nearly 5000 miles an hour. Sharply rounded in front, as if, one might fancy, the better to cleave its way, it quickly broadened, till at length a wide band of darkness covered the earth. Still no signs of a break in the clouds, and all the time the merciless metronome hammered away the precious seconds. We were helpless; all we could do was to look around us and gaze on a scene that will never fade from our memory. To the south we saw the outlines of the lofty heights of Süd-Varanger showing clear against a bright orange-coloured horizon, which looked strangely weird beneath the overhanging masses of inky, blue-black clouds. Looking along the line of totality all was shrouded in darkness, but as the eye travelled northwards, there again was that weird ochre-coloured sky, with the dark lowering canopy of those guilty clouds. And even as we looked, as if by magic, the darkness faded away as suddenly as it had come, and left us almost too

dazed to realise that all was over, and that the great eclipse of 1896 had come and gone. If anything could have consoled us for our great disappointment, it was the universal expression of sympathy that we met with, not only from our many friends and colleagues, but also from the kind-hearted Norwegians who had followed all our operations with the keenest interest, and furthered our endeavours in every possible way.

After the eclipse was over we took several photographs of the instruments *in situ*, some of which will be handed round and may help to show the arrangement of the apparatus. By the afternoon of the 11th August everything had been transported to the town of Vadsö, and on the 13th we began our homeward journey; as we had been the first astronomers to arrive, the considerable bulk of our *impedimenta* caused us to be the last to leave. That night, at Vardö, we had the great pleasure of welcoming Dr Nansen and Lieut. Johannsen on their return from their famous expedition. Journeying by way of Tröndhjem and Bergen, we reached Newcastle on the 24th August.

One result of our expedition has been to confirm Prof. Schaeberle's experience that the fixed long-focus telescope with movable slide is really one of the most valuable forms of eclipse apparatus, and, if suitably arranged, is one of the easiest to manage. Mr Ramsay's success in obtaining pictures of everything that was to be seen, under very unfavourable circumstances, abundantly confirms what has just been said. One suggestion we should like to place on record here: if a direct-vision prism of suitable dispersion were made to slide easily into position in front of the object-glass, the same long-focus telescope could be used for securing spectrograms as well as for taking pictures of the corona and prominences, devoting half the plates to one method and half to the other. In this way a single instrument could be made to yield the best results in the two most important branches of eclipse photography.

Note on the Solution of Equations in Linear and Vector Functions. By Prof. Tait.

(Read June 7, 1897.)

In a paper read to the Society on March 1 (*ante*, p. 310) I spoke of the application of some of its results to the solution of equations involving an unknown Linear and Vector Function. These results depended chiefly upon the expression of the function in terms of its roots, scalar and directional; and I now give a few instances of their utility, keeping in view rather variety of treatment than complexity of subject. The matter admits of practically infinite development, even when we keep to very simple forms of equation, and is thus specially qualified to show the richness in resources which is so characteristic of quaternions. But it will be seen also to be strongly suggestive of the extreme caution required even in the most elementary parts of this field of inquiry.

In what follows, I employ χ to denote the unknown function; ϕ, ψ, etc., known functions. ϖ is specially reserved for a self-conjugate function, and ω for a pure rotation.

1. Given
$$\phi\chi = \chi\phi \; ; \qquad \qquad (1),$$
i.e., to find the condition that two functions shall be commutative in their successive application. Let α be a root of ϕ, real or imaginary, so that
$$\phi\alpha = g\alpha \, .$$
We have at once, by applying the members of the proposed equation to α,
$$\phi\chi\alpha = \chi\phi\alpha = g\chi\alpha \, .$$
Thus, except in the case of equal roots of ϕ,
$$\chi\alpha = h\alpha \; ;$$
so that the required condition is merely that χ has the same directional roots as ϕ. When two values of g are equal, two of the directional roots of χ are limited only to lie in a definite

plane:—when all three are equal, ϕ becomes a mere magnification, and χ is, of course, wholly undetermined.

[When the roots of ϕ are all real, we have

$$S.\alpha\beta\gamma\,\chi\rho = h_1\alpha S.\beta\gamma\rho + h_2\beta S.\gamma\alpha\rho + h_3\gamma S.\alpha\beta\rho\,.$$

When two are imaginary we may preserve this form; or, if we wish to express it in terms of real quantities only, we may write it as

$$S.\alpha\beta\gamma\,\chi\rho = h_1\alpha S.\beta\gamma\rho + (h_2\beta - h_3\gamma)S.\gamma\alpha\rho + (h_2\gamma + h_3\beta)S.\alpha\beta\rho\,,$$

where the meanings of h_2, h_3, β, γ, are entirely changed.

It is well to notice that the squares of these functions preserve the form, so that in the first

$$S.\alpha\beta\gamma\,\chi^2\rho = h_1^2\alpha S.\beta\gamma\rho + h_2^2\beta S.\gamma\alpha\rho + h_3^2\gamma S.\gamma\alpha\rho\,;$$

and in the second we have the value

$$h_1^2\alpha S.\beta\gamma\rho + ((h_2^2 - h_3^2)\beta - 2h_2h_3\gamma)S.\gamma\alpha\rho + ((h_2^2 - h_3^2)\gamma + 2h_2h_3\beta)S.\alpha\beta\rho\,.$$

Thus the square roots of such expressions may be obtained by inspection.]

2. Had the known factors been different in the two members, *i.e.*, had the equation been

$$\phi\chi = \chi\psi \quad . \quad . \quad . \quad . \quad (1'),$$

the same process would still have been applicable, though the result would have been very different. For α being a root of ψ, we have

$$\phi\chi\alpha = g\chi\alpha$$

as before. But we can no longer conclude from this anything further than that the scalar roots of ψ must be the same as those of ϕ, in order that the given equation may not be self-contradictory. Thus, if ψ have three real roots, so must ϕ, and conversely. If this necessary condition be fulfilled, χ is any function which changes the *directional* roots of ψ into those of ϕ. Its own scalar roots remain indefinite.

3. Let the equation be

$$\phi\chi' = \chi\phi' . \quad . \quad . \quad . \quad . \quad (2).$$

The members, besides being equal, are conjugates; so that they represent *any* pure strain whatever.

Thus $\chi' = \phi^{-1}\varpi$, and $\chi = \varpi\phi'^{-1}$, which are of course consistent with one another. Remark that, as a particular case, ϖ may be a mere number. If ϖ be taken $= \phi\phi'$, we have the obvious solution $\chi = \phi$.

4. If we alter the order of the factors on one side of (2) we have an altogether new form :—

$$\phi\chi' = \phi'\chi \quad . \quad . \quad . \quad . \quad (3).$$

Since ϕ is given, this may be written

$$\chi' = \psi\chi$$

where ψ is known. An immediate transformation by taking the conjugate gives

$$\chi = \psi\chi\psi'$$

a type which is obviously a particular case of (1′); and, besides, will be treated later, with the sole difference that χ will then be the *given* function, and ψ that to be found. But when a solution has thus been obtained, it must be tested in the original equation; for selective eliminations, such as that just given, often introduce irrelevant solutions. (See § 8, below.)

5. A curious modification of (3) is produced by making in it ϕ and χ identical, so that it becomes

$$\chi\chi' = \chi'\chi \quad . \quad . \quad . \quad . \quad (4).$$

Though no longer linear, this equation is in some respects analogous to (1). It thus imposes the condition that χ and its conjugate shall have the same directional roots. If all three be real they must therefore form a rectangular system. If two be imaginary, the vectors of their real and imaginary parts form a rectangular system with the third. Thus χ may be any pure strain, or a rotation associated with a pure strain symmetrical about the axis of the rotation.

A simpler mode of dealing with (4) is suggested by the last remark. For we may always assume

$$\chi = \varpi\omega\,,$$

and (4) becomes

$$\varpi\omega\omega^{-1}\varpi = \varpi^2 = \omega^{-1}\varpi^2\omega\,,$$

from which (coupled with the results of (1)) the above conclusions are obvious.

6. The form

$$\chi\phi\chi' = \phi \quad . \quad . \quad . \quad . \qquad (5)$$

also admits of simple treatment. Its conjugate is

$$\chi\phi'\chi' = \phi'\,.$$

Now we can always write

$$\phi = \varpi + V\epsilon\,, \text{ with } \phi' = \varpi - V\epsilon\,,$$

and the equations above become, by addition and subtraction,

$$\chi\varpi\chi' = \varpi\,, \qquad \chi V\epsilon\chi' = V\epsilon\,.$$

Put the first of these in the form

$$\chi\varpi^{\frac{1}{2}}\omega_1.\omega_1^{-1}\varpi^{\frac{1}{2}}\chi' = \varpi^{\frac{1}{2}}\omega_2.\omega_2^{-1}\varpi^{\frac{1}{2}}$$

where ω_1 and ω_2 are, so far, arbitrary. As each side is the product of a strain and its conjugate (because the conjugate of a pure rotation is its reciprocal), we may at once write

$$\chi\varpi^{\frac{1}{2}}\omega_1 = \varpi^{\frac{1}{2}}\omega_2$$

or
$$\chi = \varpi^{\frac{1}{2}}\omega\varpi^{-\frac{1}{2}}$$

where $\omega = \omega_2\omega_1^{-1}$ is still arbitrary. To determine it, the second equation above, viz.

$$\chi V\epsilon\chi' = V\epsilon$$

gives
$$m\epsilon = \chi'\epsilon$$

where m is the product of the numerical (scalar) roots of χ; obviously unit in this case, as there is no change of volume. This gives

$$\omega\varpi^{\frac{1}{2}}\epsilon = \varpi^{\frac{1}{2}}\epsilon$$

so that the axis of ω is $\varpi^{\frac{1}{2}}\epsilon$, but the angle of rotation remains undetermined.

The direct algebraic verification of this solution is troublesome,

unless we refer the strain to the axes of its pure part ϖ, when it becomes fairly simple. For ϕ can then be written as

$$\begin{pmatrix} A^2 & -\nu & \mu \\ \nu & B^2 & -\lambda \\ -\mu & \lambda & C^2 \end{pmatrix},$$

whence it is easy to see that

$$\chi = \begin{pmatrix} e+(1-e)l^2 & \frac{A}{B}(-nf+(1-e)lm) & \frac{A}{C}(mf+(1-e)ln) \\ \frac{B}{A}(nf+(1-e)lm) & e+(1-e)m^2 & \frac{B}{C}(-lf+(1-e)mn) \\ \frac{C}{A}(-mf+(1-e)ln) & \frac{C}{B}(lf+(1-e)mn) & e+(1-e)n^2 \end{pmatrix}$$

where

$$l = A\lambda/\sqrt{A^2\lambda^2+B^2\mu^2+C^2\nu^2},\ \text{etc., and}$$
$$e^2+f^2=1.$$

7. A similar mode of treatment can, of course, be applied to the more general form

$$\chi\phi\chi' = \psi \quad . \quad . \quad . \quad . \quad (6).$$

After what has just been said, it is easy to see that if $\psi = \varpi_1 + V\epsilon_1$, we shall have

$$\chi = \varpi_1^{\frac{1}{2}}\omega\varpi^{-\frac{1}{2}},$$

with the condition for ω (and for the possibility of a solution)

$$m\omega\varpi^{\frac{1}{2}}\epsilon = \varpi_1^{\frac{1}{2}}\epsilon_1,$$

where m is the product of the numerical roots of χ.

[In connection with the results above it may be interesting to find the relations among the various constituents of the two different modes of breaking up a linear vector function into pure and rotational parts:—*i.e.*

$$\phi = \varpi + V\epsilon = \varpi_1\omega.$$

(See *Proc. R. S. E.*, vii. 316, for another solution.)

The general form of a pure rotation is

$$\omega = \alpha^{A/\pi}(\quad)\alpha^{-A/\pi} = \cos A + \sin A\, V\alpha - (1-\cos A)\alpha S\alpha$$

where α is the unit vector axis and A the angle of rotation. Thus, writing for shortness $\bar{c} = \cos A$ and $\bar{s} = \sin A$,

$$\varpi\rho + V\epsilon\rho = \bar{c}\varpi_1\rho + \bar{s}\varpi_1 V\alpha\rho - (1-\bar{c})\varpi_1\alpha S\alpha\rho$$
$$\varpi\rho - V\epsilon\rho = \bar{c}\varpi_1\rho - \bar{s}V\alpha\varpi_1\rho - (1-\bar{c})\alpha S\alpha\varpi_1\rho$$

so that

$$2V\epsilon\rho = \bar{s}(\varpi_1 V\alpha\rho + V\alpha\varpi_1\rho) + (1-\bar{c})V.V\alpha\varpi_1\alpha\,\rho\,.$$

Now Hamilton (in giving his cubic) showed that

$$(m_2 - \varpi_1)V\alpha\rho = V\varpi_1\alpha\,\rho + V\alpha\varpi_1\rho$$

so we have

$$2V\epsilon\rho = \bar{s}(m_2 V\alpha\rho - V\varpi_1\alpha\,\rho) + (1-\bar{c})V.V\alpha\varpi_1\alpha\,\rho\,;$$

and, as this is true for all values of ρ,

$$2\epsilon = \bar{s}(m_2\alpha - \varpi_1\alpha) + (1-\bar{c})V\alpha\varpi_1\alpha\,,$$

the second term disappearing when the rotation is about one of the axes of the pure part of the strain. Again

$$2\varpi\rho = 2\bar{c}\varpi_1\rho + \bar{s}(\varpi_1 V\alpha\rho - V\alpha\varpi_1\rho) - (1-\bar{c})\{\varpi_1\alpha S\alpha\rho + \alpha S\alpha\varpi_1\rho\}$$

is obviously self-conjugate.]

8. An instantaneous, and (at first sight) apparently quite different, solution of (5) is obtained by multiplying each side into the reciprocal of its conjugate. For we thus have a case of (1) in the form

$$\chi\phi\phi'^{-1} = \phi\phi'^{-1}\chi\,.$$

But this equation, which would assign to χ *any* value commutative with $\phi\phi'^{-1}$, is very much more general than (5) from which it is derived. [This is an excellent example of the necessity for caution already pointed out.]

To analyse this solution, with the view of restricting it, note that by Hamilton's method we have at once

$$m(\phi'^{-1} - \phi^{-1}) = 2V\varpi\epsilon = 2eV\varpi^{\frac{1}{2}}\alpha\,,\text{ suppose,}$$

where m is the product of the scalar roots of ϕ; α a unit vector, and e a scalar constant, both definite.

Thus $$\phi\phi'^{-1}\rho = \rho + \frac{2e}{m}\phi V\varpi^{\frac{1}{2}}\alpha\,\rho$$

$$= \rho + \frac{2e}{m}(\varpi + eV.\varpi^{-\frac{1}{2}}\alpha)V\varpi^{\frac{1}{2}}\alpha\,\rho$$

$$= \left(1 - \frac{2e^2}{m}\right)\rho + \frac{2e\overline{m}}{m}\,\varpi^{\frac{1}{2}}V\alpha\varpi^{-\frac{1}{2}}\rho - \frac{2e^2}{m}\varpi^{\frac{1}{2}}\alpha S\alpha\varpi^{-\frac{1}{2}}\rho$$

where $\overline{m}^{\frac{1}{2}}$ is the product of the scalar roots of $\varpi^{\frac{1}{2}}$, and therefore

$$m = \overline{m} - S\epsilon\varpi\epsilon = \overline{m} + e^2.$$

[The former solution, giving

$$\begin{aligned}\chi\rho &= \varpi^{\frac{1}{2}}\omega\varpi^{-\frac{1}{2}}\rho \\ &= \rho\cos A + \sin A\,\varpi^{\frac{1}{2}}V\alpha\varpi^{-\frac{1}{2}}\rho - (1-\cos A)\varpi^{\frac{1}{2}}\alpha S\alpha\varpi^{-\frac{1}{2}}\rho\,,\end{aligned}$$

contains this as a particular case, for it is easy to see that the two expressions agree if we are entitled to assume simultaneously

$$\cos A = 1 - \frac{2e^2}{m},\ \sin A = \frac{2e\overline{m}}{m},\ 1 - \cos A = \frac{2e^2}{m}.$$

The first and last are identical; and the first and second require merely that we shall have

$$1 = \left(1 - \frac{2e^2}{m}\right)^2 + \frac{4e^2\overline{m}}{m^2};$$

which is satisfied in consequence of the expression for $\overline{m}$ above.]

That the complete admissible value of χ is what we have already found, and contains only the *one* scalar indeterminate A, is easily verified by expressing χ as a linear combination of the operators 1, $\varpi^{\frac{1}{2}}V\alpha\varpi^{-\frac{1}{2}}$, $\varpi^{\frac{1}{2}}\alpha S\alpha\varpi^{-\frac{1}{2}}$, which are suggested by its relation to $\phi\phi'^{-1}$, and are obviously commutative with one another; and independent, in the sense of not producing any new operator by their combinations. Then the required relations among the coefficients are determined by comparing term by term the expressions for $\phi\chi'$ and $\chi^{-1}\phi$.

9. Finally, we may treat (5) by a method similar to that adopted for (1). Let α *now* be a directional root of χ', so that $\chi'\alpha = g\alpha$. Then we have

$$\chi\phi\alpha = \frac{1}{g}\phi\alpha\,.$$

But the cubics of χ and χ' are necessarily identical, and thus their common numerical roots can be no others than 1, g, $1/g$. Also, since ϕ is assumed to be real, g is imaginary, for ϕ changes the g directional root of χ' to the $1/g$ root of χ, and conversely.

But, if we operate by the conjugate of (5) upon α, we get

$$\chi\phi'\alpha = \frac{1}{g}\phi'\alpha\,.$$

Thus the directional roots of χ' are treated alike by ϕ' and by ϕ, and must therefore belong to $\phi^{-1}\phi'$. So those of χ belong to $\phi\phi'^{-1}$. Thus we are again conducted to the previous result; but this third method gives us great additional information as to the intrinsic nature of the strains involved, and the relations which exist among them.

10. It is, of course, only in special cases that simple methods like these can be applied to linear vector-function equations of a little greater complexity. But when they are applicable they often give singularly elegant solutions. As an instance take the equation

$$\phi_1\chi + \chi\phi_2 = \psi \quad . \quad . \quad . \quad . \quad . \qquad (7),$$

or, as it may obviously be written,

$$\chi^{-1}\phi_1 + \phi_2\chi^{-1} = \chi^{-1}\psi\chi^{-1}.$$

Let α be a directional root of ϕ_2, then at once

$$\phi_1\chi\alpha + g\chi\alpha = \psi\alpha\,,$$

or

$$\chi\alpha = (\phi_1 + g)^{-1}\psi\alpha\,.$$

If the roots of ϕ_2 be unequal, the three equations of this form completely determine χ.

11. Again, let

$$\phi_1\chi + \chi\phi_2 = \phi_3\chi\phi_4 + \psi \quad . \quad . \quad . \qquad (8).$$

If g_1, α_1, etc., are roots of ϕ_2, this gives three equations of the form

$$(\phi_1 + g_1)\chi\alpha_1 = \phi_3\chi(\phi_4\alpha_1) + \psi\alpha_1\,.$$

If the values of α be unequal, we can of course find the coefficients in

$$\begin{aligned} \phi_4\alpha_1 &= a_1\alpha_1 + b_1\alpha_2 + c_1\alpha_3 \\ \phi_4\alpha_2 &= a_2\alpha_1 + b_2\alpha_2 + \;.\;\;. \\ \phi_4\alpha_3 &= \;.\;\;.\;\;.\;\;.\;\;.\;\;. \end{aligned}$$

Then, putting λ_1 for $\chi\alpha_1$, etc., we have finally

$$\phi_3^{-1}(\phi_1 + g_1)\lambda_1 = a_1\lambda_1 + b_1\lambda_2 + c_1\lambda_3 + \phi_3^{-1}\psi\alpha_1\,.$$

The three equations of this form give λ_1, etc., that is, $\chi\alpha_1$, etc., and thus χ is found in terms of its effects on three known vectors.

12. The most general linear equation in χ and χ' may be written as

$$\Sigma\phi\chi\phi_1 + \Sigma\psi\chi'\psi_1 = \xi\,.$$

Take α, β, γ, three non-coplanar vectors, and let

$$\left.\begin{aligned}\phi_1\alpha &= p\alpha + q\beta + r\gamma\\ \phi_1\beta &= p'\alpha + q'\beta + r'\gamma\\ \phi_1\gamma &= p''\alpha + q''\beta + r''\gamma\end{aligned}\right\}\text{ etc.}$$

$$\left.\begin{aligned}\psi'\alpha &= s\alpha + t\beta + u\gamma\\ \psi'\beta &= s'\alpha + t'\beta + \;.\\ \psi'\gamma &= s''\alpha + \;.\quad.\quad.\end{aligned}\right\}\text{ etc.}$$

Apply the members of the given equation to α, β, γ separately; and operate on each of the results with $S.\alpha$, $S.\beta$, $S.\gamma$. We obtain nine scalar equations in $\chi\alpha = \lambda$, $\chi\beta = \mu$, $\chi\gamma = \nu$, of which two are

$$\Sigma(S.\alpha\phi(p\lambda + q\mu + r\nu) + S.\psi_1\alpha(s\lambda + t\mu + u\nu)) = S\alpha\xi\alpha\,,$$
$$\Sigma(S\beta\phi(p\lambda + q\mu + r\nu) + S\,\psi_1\alpha(s'\lambda + t'\mu + u'\nu)) = S\beta\xi\alpha\,.$$

These are necessary, and sufficient, to determine λ, μ, ν; and thence χ.

Notes on some Earthquakes in India. By J. W. Inglis, Mem. Inst. C.E.

(Read July 19, 1897.)

On 5th July we had the pleasure of listening to the paper on the "Calcutta Earthquake" of 12th June 1897, by Mr Heath, B.A.

It occurred to me that the Royal Society might be pleased to have a narrative account from one of its Fellows, by following up the subject. There is nothing scientific in this paper, and it is brevity itself; at the same time it may be interesting, as the facts are from personal experience of some earthquakes during my residence in India of nigh quarter of a century.

Mr Heath observed casually that a portion of the spire of the (Episcopal) Cathedral at Calcutta had fallen, and from the newspapers I read that many of the public buildings, such as the High Court of Judicature, the Museum, and many others, were badly damaged. If any one would like to see photographs of these buildings before injury took place, I thought it might be of interest to place my albums at the disposal of the President; so that, after the meeting is over, I could, if required, turn up the pages——

But, to my subject.

After the lapse of forty-one years, I read in a paper recently that the centre of the volcanic disturbance which has caused so much destruction in different parts of Bengal is definitely ascribed to a spot in the Bay of Bengal. I have always hoped that an experience which I had many years ago might some day be made clear: that pleasure, I think, I have at last attained.

During a long voyage to India in 1856, which occupied 118 days, one of not a few wonders on the deep was experienced on 21st October, when we were greatly alarmed by the ship suddenly receiving two or three severe bumps (it was in the forenoon). I can well recall the circumstance, for the sensation was exactly as if the vessel had struck a sandbank! All hands were at once summoned on deck; most of the passengers (we were about fifty

in number) rushed up to inquire the cause, for we thought the ship was stranded. Heaving the lead and other nautical examinations were duly made, after which Captain Consitt came to the conclusion that it was from the result of an earthquake at sea (or what I called a "seaquake"). Our position at the time was about Lat. 9° S., Long. 30° W., our course being in the run of the "Brazil current." The circumstance made a great impression on me. I therefore wrote it down at once, and on arrival in Calcutta in January 1857, I sent it to my much esteemed friend Sir David Brewster, in St Andrews, where I was educated.

2. During my service in India as an engineer officer in the "Public Works Department," I remember some very severe shocks of earthquake and exceptionally alarming atmospheric disturbances, such as tidal waves and storms.

In 1858 and 1860, during residence in Calcutta, I vividly recall to memory two terrible shocks of earthquake. I have no note of all the damage done, but one of them I remember, which occurred a few days after one of these shocks. It was on a Saturday that St James' (Episcopal) Church entirely collapsed. Many of the congregation were unaware of the fact, for on Sabbath, when they drove up to the forenoon service, the church was a ruin!

It was in August 1858 I witnessed the total eclipse of the sun, and the alarming inundations caused subsequently by rain and high tides from the ocean.

3. In 1863, while on duty at Lucknow, I remember an earthquake which caused the steeple of the Roman Catholic Church to settle on the south side, but after a few days, one night it collapsed eastward, and levelled the entire church in its fall.

My next experience in Oude was on 25th May 1866, being 20 odd miles from my headquarters at Sultanpur. It was a terribly hot day, the kus-kus tatties working and punka vigorously being pulled. Feeling feverish and out of sorts, I was lying down at noon and scantily clad, owing to the great heat and dust. Now and again I felt being rocked, and fancied that the "punka-walla" was amusing himself at my expense by pushing the legs of the couch with his feet. Remonstrance having failed, I jumped up, and immediately heard the roof creaking. My orderly, in the greatest alarm, rushed into the room and declared "that it was all

up, as the last day had arrived!" On my exit into the verandah, I cannot express the feeling of awe when I saw the surface of the ground. To describe the observation clearly, it was like the gentle undulations of the surface of the sea in calm weather.

My brother-in-law, who was one of my assistant engineers, was 50 miles away, and was spending the day with a number of civil and military friends at Pertabgur, when, almost within a few seconds, they experienced the same phenomena, and immediately took shelter under trees. A few hours later we experienced a terrific thunderstorm and a deluge of rain.

4. During special duty in Rangoon, one night in August 1867 (the house in which we resided being built of wood), we thought there were dacoits in the verandah, so I quietly got my revolver, but before long found that the continuous vibrations were caused by earthquake. The only damage which I can now remember was that the arches of some of the masonry buildings in the town were fissured.

5. In 1869, while engineer officer in charge of works in the Tenasserim Province of Burma, during the hot season, we experienced several shocks of earthquake. It was in August of this year there was a total eclipse of the sun. For eighteen days it had not ceased to rain: I recorded a fall during that period of 68 inches. Standing on the platform of the great pagoda, which is built on the summit of a hill, on ordinary occasions five rivers can be easily seen running their respective courses before they united with the Maulmain River. By the inundations these became one immense expanse of sea, and remained in that condition for many days. Perhaps it may not be out of place to say that I had hopes of taking some photographs of this great eclipse at Maulmain, as I was fully prepared. At Tavoy and at Mergni two of my assistant engineers were also in readiness, but the heavy and incessant rain prevented any work. Even Colonel Tennant, R.E., who had been sent by Government to the Nicobar Islands for the purpose of taking observations, was greatly disappointed by bad weather.

6. During professional residence at Delhi, in 1872; at Agra, 1875, and Cawnpore, 1879, the shocks of earthquake were also very severe; and at such seasons I always found that thunderstorms and very heavy falls of rain invariably accompanied such

disturbances. I am told by a friend who has just arrived from India that the heat this season has been exceptionally high, 97° being an average in most stations; but at Bangalore, just before the earthquake was experienced, the thermometer recorded 100°. The highest I have heard of was 127° at Jacobabad.

7. We have heard of the terrible destruction to railway communication in Assam. As one of the engineers in the construction of the first railway in Bengal, many years afterwards I was invited by a brother officer to visit the site of a bridge under construction on the Port Canning Railway, of which the ironwork and masonry had entirely collapsed from the effects of recent earthquakes. Looking at the nature of the soil in this locality of the Gangetic delta, and the rapid current in the tidal creek, it was impossible to resist a horrible shudder!

Meetings of the Royal Society—Session 1896–97.

THE 114TH SESSION.

GENERAL STATUTORY MEETING.

Monday, 23rd November 1896.

The following Council were elected :—

President.

THE RIGHT HON. LORD KELVIN, LL.D., D.C.L., F.R.S.

Vice-Presidents.

Professor JAMES GEIKIE, LL.D., F.R.S.
The Hon. Lord M'LAREN, LL.D.
The Rev. Professor FLINT, D.D.
Professor JOHN G. M'KENDRICK, LL.D., F.R.S.
Professor GEORGE CHRYSTAL, LL.D.
Sir ARTHUR MITCHELL, K.C.B., LL.D.

General Secretary—Professor P. G. TAIT.

Secretaries to Ordinary Meetings.

Professor CRUM BROWN, F.R.S.
JOHN MURRAY, Esq., D.Sc., LL.D.

Treasurer—PHILIP R. D. MACLAGAN, Esq., F.F.A.

Curator of Library and Museum—ALEXANDER BUCHAN, Esq., M.A., LL.D.

Ordinary Members of Council.

Professor T. R. FRASER, M.D.
Dr ROBERT MUNRO, M.A.
Dr D. NOËL PATON, B.Sc., F.R.C.P.E.
C. G. KNOTT, Esq., D.Sc.
Sir W. TURNER, M.B., F.R.S.
Sir STAIR AGNEW, K.C.B.
Dr JAMES BURGESS, C.I.E., M.R.A.S.
JOHN S. MACKAY, Esq., LL.D.
Professor COPELAND, Astronomer Royal for Scotland.
Professor D'ARCY W. THOMPSON.
The Rev. Professor DUNS.
Lieut.-Col. BAILEY, R.E.

Honorary Representative on George Heriot's Trust.

JOHN MURRAY, Esq., D.Sc., LL.D.

By a Resolution of the Society (19th January 1880), the following Hon. Vice-Presidents, having filled the office of President, are also Members of the Council :—

HIS GRACE THE DUKE OF ARGYLL, K.G., K.T., LL.D., D.C.L.
SIR DOUGLAS MACLAGAN, M.D., LL.D., F.R.C.P.E.

FIRST ORDINARY MEETING.

Monday, 7th December 1896.

Professor M'Kendrick, M.D., LL.D., F.R.S., Vice-President, in the Chair.

The following Communications were read:—

1. Opening Address by the Chairman:—
   (*a*) General. pp. 170–182.
   (*b*) Remarks on the Structural and Physiological Nervous Unit. pp. 182–189.
   (*c*) Experiments on the Rhythmic Stimulation of Sensory Nerves of the Skin. pp. 189–194.
   (*d*) Demonstration of the Improved Phonograph Recorder, and Remarks on the Curves thereby obtained. pp. 194–206.

2. On the Reproduction of some Marine Diatoms. By Mr G. R. M. Murray, British Museum. pp. 207–219.

3. The Eliminant of a set of Quaternary Quadrics. By Dr Thomas Muir. pp. 328–341.

4. On the Resolution of Circulants into Rational Factors. By Dr Thomas Muir. pp. 369–382.

5. On the Eliminant of $f(x)=0$, $f\left(\frac{1}{x}\right)=0$. By Dr Thomas Muir. pp. 360–368.

6. On the so-called "Hypoiodite of Magnesium." By Professor James Walker and Sydney A. Kay, B.Sc., University College, Dundee. pp. 235–248.

Mr Robert Caird, Mr James Bell Dobbie, and Mr William Sanderson were balloted for and declared duly elected Fellows of the Society.

SECOND ORDINARY MEETING.

Monday, 21st December 1896.

The Right Hon. Lord Kelvin, LL.D., F.R.S., President, in the Chair.

Mr James Bell Dobbie and Mr William Sanderson were admitted Fellows of the Society.

The following Communications were read:—

1. On Atomic Configurations in Molecules of Gases, according to Boscovich. By the President.

2. On the Retro-colic Fossæ and the Pericæcal Folds and Fossæ. By RICHARD BERRY, M.D., F.R.C.S. Ed. Communicated by Dr NOËL PATON.

3. A Research into the Chemical Nature of the Nucleins and Paranucleins of the Animal Cell. By T. H. MILROY, M.D., B.Sc. Communicated by Professor RUTHERFORD. (*Abstract.*) pp. 254–258.

4. On the Expression of any Bordered Skew Determinant as the Sum of Products of Pfaffians. By Dr THOMAS MUIR. pp. 342–359.

5. On Electrification of Air by Röntgen Rays. By The Right Hon. LORD KELVIN, Dr J. C. BEATTIE, and Dr M. SMOLUCHOWSKI DE SMOLAN. pp. 393–397.

THIRD ORDINARY MEETING.

Monday, 4th January 1897.

Professor Chrystal, LL.D., Vice-President, in the Chair.

The following Communications were read :—

1. Report of Intermediate Station on Ben Nevis. By T. S. MUIR, M.A. Communicated by Dr BUCHAN. pp. 280–296.

2. On Intermediary Links between Man and the Lower Animals. By Dr MUNRO. (*Abstract.*) pp. 249, 250.

FOURTH ORDINARY MEETING.

Monday, 18th January 1897.

Sir Arthur Mitchell, K.C.B., LL.D., Vice-President, in the Chair.

The following Communications were read :—

1. On the Ocean Ranger Reef of the South-West Pacific. By Dr JOHN MURRAY.

2. On the Physical Conditions of the Ocean to the East of the Australian Continent. By Dr JOHN MURRAY.

3. Further Note on Magnetic Strains. By Dr C. G. KNOTT.

4. On the Physical Properties of the Electro-magnetic Medium. By Professor TAIT.

5. On Osmotic Pressure against an ideal Semi-permeable Membrane. By the Right Hon. LORD KELVIN, President. pp. 323–325.

6. On a Differential Method for Measuring Differences of Density and of Vapour-Pressures of Liquids at one Temperature and at Different Temperatures. By the Right Hon. LORD KELVIN. pp. 429–432.

FIFTH ORDINARY MEETING.

Monday, 1st February 1897.

The Right Hon. Lord Kelvin, President, in the Chair.

The following Communications were read :—

1. The Changes in the Mucosa of the Corpus Uteri, and in the attached Fœtal Membranes, during Pregnancy. By J. CLARENCE WEBSTER, M.D., F.R.C.P. Ed.
2. A very simple Logical Machine. By Professor D'ARCY THOMPSON.
3. On the Conductive Effect produced in Air by Röntgen Rays, and by Ultra-Violet Light. By the Right Hon. LORD KELVIN, Dr BEATTIE, and Dr SMOLAN. pp. 406–417.
4. Crystallization according to Rule. By the Right Hon. LORD KELVIN.
5. Note on the Sensitiveness of the Skin to Weak Electric Currents as compared with the Sensitiveness of a Telephone to the same Currents. By Professor J. G. M'KENDRICK. pp. 251–253.

Mr JAMES ROBERT ERSKINE-MURRAY and Dr A. LOCKHART GILLESPIE were balloted for, and declared duly elected Fellows of the Society.

SIXTH ORDINARY MEETING.

Monday, 15th February 1897.

Professor Geikie, Vice-President, in the Chair.

Dr A. LOCKHART GILLESPIE was admitted a Fellow of the Society.

The following Communications were read :—

1. On the Electrolysis of Potassium Ethyl-sulphone-Acetate. By Dr CRUM BROWN, F.R.S., and Dr BOLAM. p. 297.
2. On Configurations of Minimum Potential Energy in Clusters of Homogeneous Molecules, with application to the Theory of Crystalline Forms. By the Right Hon. LORD KELVIN.
3. On Apparent and Real Dis-electrification of Solid Dielectrics produced by Röntgen Rays and by Flame. By the Right Hon. LORD KELVIN, Dr BEATTIE, and Dr SMOLAN. pp. 397–403.
4. On Photo-Chemical Action. By Professor GIBSON. pp. 303–309.
5. On the Compressibility of Salt Solutions. By Professor TAIT.

SEVENTH ORDINARY MEETING.

Monday, 1st March 1897.

The Right Hon. Lord Kelvin, President, in the Chair.

The following Communications were read :—

1. The CHAIRMAN showed Models illustrating the Dynamical Theory of Hemihedral Crystals.

2. On the Meteorology of Edinburgh, Part II. By Mr R. C. MOSSMAN. *Trans.* xxxix. pp. 63–207.

3. On the Linear and Vector Function. By Professor TAIT. pp. 310–312.

4. By permission of the Meeting. On the Influence of Röntgen Rays in respect to Electric Conduction through Air, Paraffin, and Glass. By the PRESIDENT, Dr J. C. BEATTIE, and Dr SMOLUCHOWSKI DE SMOLAN. pp. 403–406.

5. By permission of the Meeting. On Electric Equilibrium between Uranium and an insulated Metal in its neighbourhood. By the PRESIDENT, Dr J. C. BEATTIE, and Dr SMOLUCHOWSKI DE SMOLAN. pp. 417–428.

Mr WALTER BIGGAR BLAIKIE, Dr JOHN CARRUTHERS BEATTIE, Dr HARRY RAINY, Dr RICHARD J. BERRY, Dr THOMAS DUNCAN GREENLEES, Dr ALEXANDER CRUIKSHANK HOUSTON, and Mr W. OWEN WILLIAMS, were balloted for and declared duly elected Fellows of the Society.

ALEXANDER AGASSIZ, E.-H. AMAGAT, STANISLAO CANNIZZARO, GABRIEL LIPPMANN, FRIDTJOF NANSEN, FERDINAND VON RICHTHOFEN, HENRY A. ROWLAND, GIOVANNI V. SCHIAPARELLI, and FERDINAND ZIRKEL were, on the proposal of the Council, balloted for and declared duly elected Foreign Honorary Fellows of the Society.

EIGHTH ORDINARY MEETING.

Monday, 15th March 1897.

The Right Hon. Lord Kelvin, President, in the Chair.

Dr JAMES BUCHANAN YOUNG, Mr W. B. BLAIKIE, Dr HARRY RAINY, and Dr RICHARD J. BERRY, were admitted Fellows of the Society.

The following Communications were read :—

1. On Deaf Mutism and its Prevention. By JAMES KERR LOVE, M.D. Communicated by Professor M'KENDRICK, F.R.S.

2. Note on an Analysis of Human Gastric Juice. By W. R. LANG, B.Sc. Communicated by Professor M'KENDRICK, F.R.S. p. 298.

3. On the Structure and Origin of Coral Reefs, Part I. By Dr JOHN MURRAY.

4. A New Form of Constant Volume Air Thermometer, which shows the Total Pressure directly, and may be graduated in degrees by Temperature. By Mr J. R. ERSKINE-MURRAY. pp. 299–302.

The Very Rev. Principal CAIRD, Professor GEORGE DARWIN, and Sir WILLIAM FLOWER, K.C.B., were, on the proposal of the Council, balloted for and declared duly elected as British Honorary Fellows of the Society.

NINTH ORDINARY MEETING.

Monday, 5th April 1897.

Professor M'Kendrick, Vice-President, in the Chair.

The following Communications were read :—

1. On Electrical Properties of Fumes proceeding from Flames and burning Charcoal. By the Right Hon. LORD KELVIN and Dr MAGNUS MACLEAN. pp. 313–322.

2. The Automorphic Linear Transformation of a Quadric. By Dr THOMAS MUIR. *Trans.* xxxix. pp. 209–230.

3. On Ethane prepared from Ethyl-Iodide, and on the Properties of some mixtures of Ethane and Butane. By Professor J. P. KUENEN. pp. 433–442.

4. Continuation of Experiments on Electric Properties of Uranium. By the Right Hon. LORD KELVIN, Dr J. CARRUTHERS BEATTIE, and Dr M. SMOLUCHOWSKI DE SMOLAN. pp. 417–428.

5. Our Perception of the Direction of Sound. By ALBERT A. GRAY, M.D. Communicated by Professor M'KENDRICK. pp. 443–452.

6. On the General p, v, t Equation. By Professor TAIT.

7. Note on the Reducing Power of the Living Animal Tissues. By DAVID FRASER HARRIS, M.B., B.Sc. pp. 383, 384.

8. Haematoporphyrinuria, and its Relations to the Origin of Urobilin. By the Same. pp. 385–390.

9. The Diurnal Variation in the Frequency of Storms on Ben Nevis Observatory during the 13 years 1894–86. By Mr A. RANKIN. Communicated by Dr BUCHAN.

TENTH ORDINARY MEETING.

Monday, 3rd May 1897.

Professor Chrystal, Vice-President, in the Chair.

The following Communications were read :—

1. Dschâbir Ben Hayyán and the Chemical Writings ascribed to him. By Professor JOHN FERGUSON.

2. The Seasonal Changes in the Pressure and Temperature of the Atmosphere from May to June and November to December. By Dr Buchan.
3. On Supersaturation. By W. W. J. Nicol, M.A., D.Sc. pp. 473–480.
4. On the Geometrical Investigation of the Circular Functions of 3θ and 5θ. By Professor Anglin, LL.D. pp. 453–456.
5. On some Nuclei of Cloudy Condensation. By John Aitken, F.R.S. *Trans.* xxxix. pp. 15–25.

The following Candidates for Fellowship were balloted for and declared duly elected Fellows of the Society:—

Mr John William Shepherd.
Mr Alfred George Nash, B.Sc.

ELEVENTH ORDINARY MEETING.

Monday, 17th May 1897.

Sir Arthur Mitchell, K.C.B., Vice-President, in the Chair.

The following Communications were read:—

1. Obituary Notice of the late Dr E. Sang. By D. Bruce Peebles. pp. xvii.–xxxii.
2. Dschâbir Ben Hayyán and the Chemical Writings ascribed to him. Part II. By Professor John Ferguson.
3. On some Type Specimens of Lepidoptera and Coleoptera in the Edinburgh Museum of Science and Art. By P. H. Grimshaw, F.E.S. Communicated by Dr Ramsay H. Traquair, F.R.S. *Trans.* xxxix. pp. 1–11. (*Abstract.*) pp. 326, 327.
4. On a New Variety of *Hestina nama*, Dbl. By the Same. *Trans.* xxxix. pp. 13,14.

TWELFTH ORDINARY MEETING.

Monday, 7th June 1897.

Professor James Geikie, F.R.S., Vice-President, in the Chair.

Dr John Carruthers Beattie and Mr Robert Caird were admitted Fellows of the Society.

The Chairman announced that the Council had awarded:—

1. The Gunning Victoria Jubilee Prize for 1893–96 to Mr John Aitken, for his brilliant investigations in Physics, especially in connection with the formation and condensation of aqueous vapour.

2. The Keith Prize for 1893–95 to Dr CARGILL G. KNOTT, for his papers on the Strains produced by Magnetism in Iron and in Nickel, which have appeared in the *Transactions* and *Proceedings* of the Society.

3. The Mackdougall-Brisbane Prize for 1894–95 to Professor JOHN G. M'KENDRICK, for numerous Physiological papers, especially in connection with Sound; many of which have appeared in the Society's publications.

4. The Neill Prize for 1892–95 to Mr ROBERT IRVINE, for his papers on the action of Organisms in the Secretion of Carbonate of Lime and Silica, and on the solution of these substances in Organic Juices. These are printed in the Society's *Transactions* and *Proceedings*.

The following Communications were read :—

1. A Contribution to the Comparative Anatomy of the Mammalian Organ of Jacobson. By R. BROOM, M.D., D.Sc. Communicated by Sir WILLIAM TURNER, *Trans.* xxxix. pp. 231–255. (*Abstract.*) pp. 391, 392.

2. On Magnetic Strains. By Professor C. G. KNOTT, D.Sc.

3. On the Solution of Equations connecting Linear Vector Functions. By Professor TAIT. pp. 497–505.

4. On the Electrification of Air by Uranium and its compounds. By J. C. BEATTIE, D.Sc. With a note by the Right Hon. LORD KELVIN. pp. 466–472.

5. On simple Formulæ giving Approximate Values of the Roots of the Bessel Function of Order n and its first derived function, in terms of the roots of (say) $J_2(x)=0$, $J'_3(x)=0$ (n even), or those of $J_3(x)=0$, $J'_4(x)=0$ (n odd). By Dr W. PEDDIE.

Mr JAMES A. MACDONALD, M.A., B.Sc., Mr HUGH DAVIDSON, Dr ANGUS M'GILLIVRAY, Mr GEORGE ARTHUR MITCHELL, M.A., Dr JOHN GORDON GORDON-MUNN, and Dr RICHARD JOHN LLOYD were balloted for and declared duly elected Fellows of the Society.

THIRTEENTH ORDINARY MEETING.

Monday, 5th July 1897.

The Hon. Lord M'Laren, Vice-President, in the Chair.

Mr JAMES A. MACDONALD and Dr ANGUS M'GILLIVRAY were admitted Fellows of the Society.

The following Communications were read :—

1. Note on the Calcutta Earthquake (June 12, 1897), as recorded by the Bifilar Pendulum at the Edinburgh Royal Observatory. By Mr THOMAS HEATH, B.A. pp. 481–488.

2. Leakage from Electrified Metal Plates and Points, placed above

and below uninsulated Flames. By the Right. Hon. LORD KELVIN and MAGNUS MACLEAN, D.Sc.

3. The Antivenomous Properties of the Bile of Serpents and other Animals ; and an Explanation of the Insusceptibility of Animals to the Poisonous Action of Venom introduced into the Stomach. By Professor FRASER, M.D. (*Abstract.*) pp. 457–465.

4. The Influence of Excessive Muscular Work on the Metabolism. By Drs DUNLOP, NOËL PATON, STOCKMAN, and Mr IVISON MACADAM.

5. The Development of the Müllerian Ducts of Reptiles. By GREGG WILSON, B.Sc., Ph.D. (Communicated by Professor J. COSSAR EWART, F.R.S.)

6. The C Discriminant as an Envelope. By J. A. MACDONALD, M.A., B.Sc. *Trans.* xxxix. pp. 27–32.

FOURTEENTH AND LAST ORDINARY MEETING.

Monday, 19*th July* 1897.

The Right Hon. Lord Kelvin, President, in the Chair.

PRIZES.

The Keith Prize for 1893–5 was presented to Dr CARGILL G. KNOTT, for his papers on the Strains produced by Magnetism in Iron and in Nickel, which have appeared in the *Transactions* and *Proceedings* of the Society.

The President, on presenting the Prize, said :—

The various results of Dr Knott's investigations on Magnetic Strains have been communicated from time to time to the Royal Society of Edinburgh, and are embodied in five papers, four of which are published in the *Transactions.* The fifth is now passing through the press. A sixth will be communicated to the Royal Society this evening.

I. *On Superposed Magnetisms in Iron and Nickel.* Read July 2, 1883; vol. xxxii. pp. 193–203.

In this paper the Wiedemann effect—that is, the twist produced in a longitudinally magnetised wire when a current is passed along it—is studied. The effect in nickel is shown to be opposite to that in iron.

II. and III. *On some relations between Magnetism and Twist in Iron and Nickel.* Part I. read July 16, 1888; vol. xxxv. pp. 377–390. Parts II. and III. read June 1, 1891; vol. xxxvi. pp. 485–535.

In these papers many new facts in the subject of magnetic strains are recorded. The chief points are: the coordination of the Joule and Wiedemann effects; the discovery and explanation of a *maximum twist* in nickel, although there is no *maximum elongation*, in moderate fields; the observation of the Wiedemann effect in cobalt; the effect of tension on the Wiedemann effect, and therefore, by inference, on the Joule effect; the nature of the hysteresis in the Wiedemann effect as either of the magnetising forces is taken through a complete cycle; the curious difference in the amount and occasionally in the direction of the twist according to the order in which the magnetising forces are applied; the magnetic polarity produced by twisting wires conveying currents; the reversal *with the current* of the polarity acquired by twisting; the residual character of this polarity and its remarkably high value, etc. etc.

IV. *The Strains produced in Iron, Steel, and Nickel Tubes in the Magnetic Field.* Part I. read January 6, 1896; vol. xxxviii. pp. 527–555.

V. Part II. read June 7, 1897 (not yet published).

A novel line of research is followed out in these communications, which have to do mainly with the effect of magnetism in altering the internal volume of tubes of iron and nickel. The effect was first noticed in May 1891, in Japan; and the results were communicated to the Royal Society of Edinburgh in July of that year (see *Proceedings*, vol. xviii. pp. 317–9). The subsequent experiments, for the carrying out of which tubes of iron, steel, and nickel, of suitable form and various bores, were specially prepared, were all made in the Physical Laboratory of the University of Edinburgh.

The most important experiments are those that have to do with the behaviour of a series of tubes of each metal of gradually

widening bore,—the tubes being, in fact, produced by successive borings from the same original bar.

Broadly speaking, the tendency in the case of the first set of iron and steel tubes was to show decrease of volume in low fields and increase of volume in moderate and high fields; but in the last set of iron tubes studied, decrease of volume was obtained in high fields also.

The nickel tubes all agreed in showing large diminution of volume in most fields, with slight increase of volume in very low fields.

As a rule, the wider the bore of the tube the greater the maximum change of volume. There are, however, curious deviations from this rule, which are shown in Part II. to be due to the fact that the volume change is a differential effect, being the resultant of a change of length in the direction of magnetisation, and a change of area transverse thereto. These longitudinal and transverse dilatations tend to be of opposite sign, and experience interesting variations as the bore of the tube is made wider and wider.

The volume changes measured in these experiments are much greater than those shown by iron or nickel rods, and indicate a very complex condition of strain, the investigation of which seems to be beyond our present powers of analysis.

For these novel and highly interesting results, obtained by experimental investigation carried on through six years with remarkable ability and perseverance, the Keith Prize, for the period 1893–5, has been awarded by the Council to Dr Cargill G. Knott.

The Makdougall-Brisbane Prize for 1894–6 was presented to Professor JOHN G. M'KENDRICK, for numerous Physiological papers, especially in connection with Sound, many of which have appeared in the Society's publications.

The President, on presenting the Prize, said:—

During the last four years Professor M'Kendrick has devoted himself with persevering zeal and enthusiasm to researches in the physiological and dynamical actions concerned in the perception of sound. The admirable model with which he explained and demonstrated by actual working, a mechanism in accordance with

Helmholtz's theory of the mechanism of the ear for recognising different musical notes, is a valuable contribution to science and to scientific teaching.

A first instalment of his work on the phonograph, given at the request of the Council, as the sole business of the meeting of the Royal Society here on the 27th of November 1894, is still remembered with lively interest by all who heard it. Farther investigations followed, and a general account of results is published in the *Trans. Roy. Soc. Edin.* for 1896 (vol. xxxviii. part 4). His later work on the Fourier analysis of curves obtained by mechanical magnification of the traces on the wax cylinder of the phonograph, from the different vowel sounds, has already given interesting results, and is well adapted to aid in the discovery of the real character of that marvellous system of varieties of sound which constitutes speech.

For these important and interesting researches on Sound, and the Physiology of the Perception of Sound, the Makdougall-Brisbane Prize for 1894–6 has been awarded to Professor M'Kendrick.

The Neill Prize for 1892–5 was presented to Robert Irvine, Esq., for his papers on the action of Organisms in the Secretion of Carbonate of Lime and Silica, and on the Solution of these substances in Organic Juices. These are printed in the Society's *Transactions* and *Proceedings*.

The President, on presenting the Prize, said:—

The Council of the Royal Society of Edinburgh has awarded the Neill Prize for the period 1892–5 to Mr Robert Irvine for his valuable researches:—

1. On the Secretion of Carbonate of Lime and Silica by Organisms.
2. On the Solution of Carbonate of Lime by Sea-Water.
3. On the Presence of Manganese Peroxide in Marine Deposits. And
4. On the Composition of Sea-Water Salts.

These researches were carried out during a long series of years, in conjunction with Dr John Murray, Dr Sims Woodhead, Dr

John Gibson, the late Mr George Brook, and Mr William Anderson. The papers describing the results have been published from time to time in the *Transactions* and *Proceedings* of the Society, and have most intimate and important connection with many Oceanographical Problems.

The following Communications were read :—

1. Notes on some Indian Earthquakes. By J. W. INGLIS, Memb. Inst. C.E. pp. 506–509.

2. Relations among various Types of Magnetic Strain. By C. G. KNOTT, D.Sc.

3. Notes on the Total Eclipse of the Sun, 8th August 1896. By the ASTRONOMER ROYAL FOR SCOTLAND and Mr A. J. RAMSAY. pp. 489–496.

Donations to the Library of the Royal Society from 1895 to 1897.

I. Transactions and Proceedings of Learned Societies, Academies, &c.

Adelaide.—Royal Society of South Australia. Transactions and Proceedings. Vols. XIX.–XXI. 1. 1895–97. 8vo.

Observatory. Meteorological Observations, 1886–93. 3 Vols. 4to.

American Association for the Advancement of Science.—43rd Meeting (Brooklyn, 1894). 44th (Springfield, Mass., 1895). 45th (Buffalo, N. Y., 1896).

Amsterdam.—Kon. Akademie van Wetenschappen. Verhandelingen. Afd. Natuurkunde. 1ste Sectie. Deel II., III., V. 1894–97. 2te Sectie. Deel II. No. 2, IV., V. 1894–97.—Afd. Letterkunde. Deel I. 4–6. 1894–97.—Verslagen en Mededeelingen. —Letterkunde. Register. Deel I.–XII. 8vo. 1897. Verslagen der Zittingen van de Wis-en Naturkundige Afdeeling. 1893–97. 4 Vols.—Jaarboek, 1894–96.—Poemata Latina.

Wiskundig Genootschap. Nieuw Archief voor Wiskunde. 2e Reeks, Deel I. 2, III. Opgaven VI. 5, 6, VII. 1–3.—Revue Semestrielle des Publications Mathématiques. Tom. IV., V. 1. 1895–97.

Flora Batava. 309–318 Afleveringen. (*From the Dutch Government.*)

Australia.—Australasian Association for the Advancement of Science. Report, 6th Meeting (Brisbane), 1895. 8vo.

Baltimore.—Johns Hopkins University. American Journal of Mathematics. Vols. XVII., XVIII., XIX. 1, 2. 1895–97. 4to.—American Chemical Journal. Vols. XVII., XVIII., XIX. 1, 2. 1895–97.—American Journal of Philology. Vols. XV. 2–4, XVI., XVII. 1–3. 1895–97.—University Studies in Historical and Political Science. 13th and 14th Series.—University Circulars. Nos. 115–120. 1895–97.

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OBITUARY NOTICES.

Benjamin Carrington, M.D., M.R.C.S., F.R.S.E., Corr. Mem. Linn. Soc., N.S.W.; Corr. Mem. R.S. Tasm. By William Henry Pearson, Knutsford.

(Read December 16, 1895.)

Dr Carrington was born on January 18th, 1827, at Lincoln, and received his early education in the neighbourhood; afterwards he was articled at Liverpool to Dr M'Nicoll, with whom he lived for some years.

Dr M'Nicoll was an enthusiastic naturalist and a great lover of poetry; under his influence probably was created or developed the love of natural history which the subject of our memoir was famed for. From Liverpool he proceeded to Edinburgh University. Whilst studying there he wrote a monograph of the British Grasses, and illustrated it with a set of specimens, with dissections of the minuter organs so beautifully and accurately prepared that they won for him the admiration of the leading botanists of the University. Here he made the acquaintance of Greville, Hooker, and Balfour, and no doubt his life's devotion to cryptogamic botany was influenced originally by Greville and Hooker. For the former he had the most profound admiration.

He was a thorough naturalist. There was no object in nature but excited his curiosity and attention: the more minute, the more careful. For some time he practised at Southport. Whilst there the annelids occupied his attention; and a paper on the Southport species, read before the British Association at one of its meetings, shows the intimate knowledge he had of them. He contributed papers to various magazines, and assisted specialists in this branch of natural history, and one species was, I believe, named in his honour. He had an intimate acquaintance with the British Flowering Plants,

and freely helped other botanists with his knowledge; also an accurate knowledge of the British Lichens, adding several new species to our Flora, corresponding with Leighton, Mudd, and other contemporary authorities. His knowledge of the British Mosses made him a no mean, though friendly, rival of Wilson, Spruce, and other leading authorities. He contributed several papers to the *Phytologist*, *Proceedings of the Botanical Society of Edinburgh*, and other scientific journals, his most important papers being on the British *Orthotrica*.

It will, however, be by his devotion to the study of the Hepaticæ that his name will especially be remembered.

He practised first at Radcliffe, near Manchester; then in succession at Lincoln, Yeadon, Southport, and Eccles.

In 1861 he visited the south of Ireland, his chief object being, as he says (after the renovation of his health), the collection and study of the Hepaticæ. The results of this visit was the appearance of his interesting "Gleanings among the Irish Cryptogams," published in the *Trans. Bot. Soc. Edin.* in 1863; an extensive list of lichens, mosses, and hepaticæ, with valuable notes on many species, especially of the latter order. It is illustrated by two beautiful plates, which indicate the skill he had attained in the art of delineating cryptogamic plants. Another result of this visit to Ireland was the rich contribution he made to Rabenhorst's *Bryotheca europeœ*, and Gottsche and Rabenhorst's *Hepaticœ europeœ*, one part of the latter being almost composed of the doctor's collecting.

In 1862 appeared Miall and Carrington's *Flora of the West Riding*, for which he compiled the list of cryptogams.

About this time he began to prepare a work on the British Hepaticæ, corresponding with all collectors and those interested in this group: De Notaris, Gottsche, and Lindberg on the Continent; Wilson, Hooker, Spruce, and others here.

In 1874 appeared the first part of what promised to be the most important work since the publication of Hooker's magnificent *British Jungermanniœ*, in 1816. Three further parts were issued in 1875 and 1876. The fourth had an ominous note appended, which stated that in consequence of the indisposition of the author the letterpress was some pages short. For some time he continued in

a very low state of health, and about the years 1880 and 1881 had to undergo several painful operations, under which his friends were afraid he would succumb. Thanks, however, to the skill of a Manchester surgeon, in whose care he was, and for whom he had the most grateful regard, he rallied, and was for several years longer able to pursue his favourite studies, but never with the same ardour; and what with difficulties with his publisher, and his enfeebled health requiring conserving to continue his professional duties, he seemed to shrink from the task of completing his beautiful and valuable work, although friends had proffered to assist him.

In 1876 he spent some time in the neighbourhood of the Trossachs, and there made what Dr Spruce describes as one of his happiest finds, *Hygrobiella myriocarpa.* This he published, with several other new species, in the *Trans. Bot. Soc. Edin.*, vol. xiii., 1879.

In 1878 we issued the first part of our *Hep. Brit. Exsicc.*, in the preparation of which Dr Carrington took great delight. Four fasciculi were issued, representing nearly all the British species, and between 17,000 and 18,000 specimens distributed.

In 1886 two Manchester botanical friends, who had gone to the Antipodes—Mr Thomas Whitelegge to New South Wales, Mr R. Bastow to Tasmania—sent large collections of hepaticæ, which we studied together. The results were published: those of Mr Whitelegge's collection in *Pro. Linn. Soc., N.S.W.*, illustrated by twelve plates, the cost of which was generously defrayed by the late Sir William MacLeay; those of Mr Bastow in the *Proc. Roy. Soc. of Tasmania* for 1887. These were the two last papers published by Dr Carrington.

In the same year he was elected a Corresponding Member of the Linnean Society of New South Wales and of the Royal Society of Tasmania. On the resignation of the first President, Mr John Whitehead, he was elected President of the Manchester Cryptogamic Society, which position he held till his death.

In 1874 he was elected F.R.S.E., and he was at one time F.L.S.

The following British Hepaticæ were either found or identified as British by him:—

Cesia crenulata (Gott.), sent to Dr Gottsche as a new species.

Cesia corallioides (N.), detected under the name of *C. concinnata* in Dr Greville's herbarium.

Cesia crassifolia (Carr.), collected near Ben Lawers by the late Dr A. O. Black.

Marsupella sphacelata (Giesecke), collected by the late G. E. Hunt on Ben MacDhui and Loch Kandor, 1868.

Marsupella Nevicencis (Carr.), collected on Ben Nevis by Mr John Whitehead, July 1875.

Scapania Bartlingii (Hampe), first recorded as British, from specimens collected on rocks near the Strid, Bolton Woods, Yorkshire, 1858.

Hygrobiella myriocarpa (Carr.). Spruce, discovered near Ben Venue, July 1876.

Riccia glaucescens, Carr., discovered at Barmouth, North Wales.

Riccia tumida, Lindenb., collected by Mr Joshua near Monmouth, May 1877.

Riccia sorocarpa, Bischoff, collected by B. M. Watkins on Great Doward Hill, near Ross.

One of our rarest and most beautiful hepatics was named in his honour by the late Professor Balfour; and Herr J. B. Jack, in his monograph of the European *Radulæ*, named one of the rarest *Radula Carringtonii* after him. The late Professor Lindberg founded a new genus, which he named *Carringtonia*.

About twelve months before he died, his valuable collection was acquired for the Manchester Museum by the Owens College authorities, and under the care of Professor F. E. Weiss it has been arranged, and is now accessible to students.

Dr Carrington was a widely-read man, passionately fond of poetry, his favourite authors being Keats, Shelley, and Wordsworth, having no mean skill himself in the accomplishment of verse, but the "nice backwardness afraid of shame" withheld him from publishing more in this field as in scientific ones. Of an extremely retiring disposition, meek and gentle in spirit, the memory of him will be treasured by all who were fortunate enough to have known him.

On the 18th of January 1893, his sixty-sixth birthday, he died at Brighton, and was buried in the Carlton Hill Cemetery in that town.

The following is a list of his most important contributions to science, taken from Professor Underwood's admirable *Index Hepaticarum* ; — "*Memoirs*," *Torrey Bot. Club*, vol. iv. No. 1 (1893).

"Gleanings among the Irish Cryptogams," *Trans. Bot. Soc. Edin.*, vii. 370–372, 379–388 (1863).

"Irish Hepaticæ," *Trans. Bot. Soc. Edin.*, vii. 441–458 ; Pl. X., XI. (1863).

"On Two Hepaticæ new to Britain ; *Jungermannia saxicola, J. Bartlingii*," *Manchester Lit. Phil. Soc. Proc.*, iv. 186–188 (1867).

"Hepaticæ in Robert Brown's *Florula discoana*," *Trans. Bot. Soc. Edin.*, ix. 453, 454 (1868).

List of Greenland Hepaticæ.

"Dr Gray's Arrangement of the Hepaticæ," *Trans. Bot. Soc. Edin.*, x. 305–309 (1870).

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Professor John Stuart Blackie.

By the Rev. Walter C. Smith, D.D., LL.D.

(Read February 3, 1896.)

John Stuart Blackie was certainly the most outstanding figure in Scotland as the 19th century drew to its close. His fine features and plaided form were familiar to Scotchmen all the world over, recognisable also by Englishmen and Germans who took any interest in the literature of our country. We were all proud of the veteran scholar and author; but our love for the man was more than our pride in his attainments, for his was a character that attracted the affection of all who knew him intimately. In spite of a strong fighting propensity, leading him to denounce snobs and West-ends, and to run tilt at our self-satisfied ignorance, and in general to hit out at anything, however respectable, which he did not approve of, yet essentially his was a very loving nature, and he was in consequence very greatly loved. It is not inconsistent with this that he sometimes, in the heat of the moment, unthinkingly touched men's sore points, and pained them, when he did not mean it. But he never deliberately wounded any one, or spoke a word inconsistent with "the charity that thinketh no evil." His singular purity of mind also was almost feminine in its sweetness and delicacy; yet there was nothing effeminate about him: he was every inch a Man. His patriotism was perhaps, at times, somewhat effusive, yet it was not blinded by prejudice, for he could see, and did not spare, the faults of his countrymen; and if he loved a Scottish song above all others, he was not without a good reason for his preference. Altogether, he was a frank, outspoken, right-hearted Scot, whose virtues and blemishes were both equally manifest; but the former were of high excellence, and the latter, at the worst, were but the follies of a rather "old boy," as he was wont to call himself.

It is not easy exactly to define what were the grounds of the

high position he had assuredly attained as representing the literature of Scotland during these latter days. There were more than one of his fellow countrymen who had reached a higher eminence than he; but they had migrated to England, and were counted among its celebrities. His work, however, was all done in the city which Scott had made so illustrious, and where Jeffrey had sharpened his pen, and not many remained now to vindicate its former name. Wilson had departed, and Aytoun followed him soon; but for eighty-six summers Blackie lived on, his restless and versatile genius standing almost alone. That naturally drew special attention to him, though it will not account for the place he held in the hearts of the people. Nor is it easy to say what was the root of that reverence which gathered so many thousands from all parts of the country to witness his funeral, and to sorrow at his departure. Though he filled a Professor's chair for so many years, he was not a great scholar either of the English, or the German, type; but had a great scorn for mere niceties of grammar, and would have rollicked in boisterous fun at the idea of being the man who knew the mystery of the "enclitic δε." Yet he was familiar with the great classics and loved them; and if not many thorough Grecians came from his classes, not a few men found there what is of more moment, a genuine taste for letters, and the fine sense of cultured thought. Though he wrote and spoke not a little on political questions—on intricate land-laws, *e.g.*—yet he was no politician, and hardly ever read a newspaper unless it happened to contain in its columns a letter from himself. Aristotle's Politics he knew, but, I think, he had given no heed to the history of English political thought. Yet he touched, now and then, with a gleam of true light, questions of deep concern to the poor, and made them feel that, if he had no precise plan for the bettering of their lot, he was, at least, deeply concerned about their sufferings. Again, though his views latterly were nowise in harmony with the Evangelical Theology of the Scottish people, yet he had once come under its influence, and that spell continued to hold him to the end, so that he never found any other worship that gave equal expression to his deepest feelings, and he never ceased to glory in the heroic history of its covenanted martyrs. Probably the heterodox opinion was condoned by the historic

enthusiasm. At any rate, the Professor's Theology was never taken so seriously as his admiration for all that was heroic in the Fathers of the Scottish Church from John Knox to Richard Cameron. Blackie had it in him to be a considerable Lyric Poet, for some of his songs contain lines, and even verses, with the true ring of a man's heart in them. But he had not patience to point and polish them: the gem was there, but it was not cut so as to bring forth its perfect lustre. Probably he himself would have rested his claims to be remembered rather on his philosophy than on his poetry; for though he had none of the Scottish love of metaphysics, and had never mastered the systems of Hume, or Hamilton, or Kant, yet he spoke many a wise and true word to his countrymen, not merely in his book on *Self Culture* and his *Four Phases of Morals*, but in almost every one of the many lectures he delivered on all kinds of subjects. These, indeed, were often marred to those who had not heard them by the almost boyish freaks which, down to the last, he indulged in when addressing an audience, and which were always carefully reported so as to make him often look liker a mountebank than a philosopher. He used to complain of this occasionally, and I had to tell him he had only himself to blame, for he spoke more sense and also more nonsense than any other man I knew. But away from the excitement of public meetings, in the quiet hour, *e.g.*, of a Sabbath evening, such as I often spent with him in his latter days, it was good to listen to his subdued and chastened thought, lit up by many an apt quotation from Aristotle, or Goethe, or the Bible, and one could easily understand, at such times, his claim to be reckoned a philosopher. In those quiet hours his natural eloquence was very impressive, for he was a born orator; and if he could have been kept from skipping and romping away from his theme, at other times, he could not have failed to win the brilliant, if evanescent, honours of a great public speaker. Take him for all in all, he was a man of fine gifts and versatile powers, genuine and right-hearted, who but that he yielded to that versatility of mind might have been a notable poet, orator, or philosopher, but was only Blackie, whom all his countrymen loved. His language was often egotistic, and yet he was not vain, for at bottom his character was simple and humble. The source of not a little fun and light-

hearted mirth, yet he had no humour, I think, and could hardly tell a story without missing the point. He often startled pious folk by "speaking unadvisedly with his lips," and yet there was in his heart a profound reverence for all things good and holy. As I recall him to-day, I seem to be dealing with a lot of contradictory elements, which nevertheless were all sweetly harmonised in a generous and beautiful and loveable personality. A deft and nimble thinker, he was yet profoundly serious, and constantly brooding on the weightiest concerns. Careless of conventionality, and often startling ordinary folk, he was yet at bottom pious and touchingly reverent, loving Socrates and exalting Goethe, but never speaking of Jesus except, in a subdued tone, as "our Saviour." To the last year of his life he was always ready to travel, in wintriest weather, a hundred miles, that he might speak or lecture to some poor villagers, to brighten an evening for them with wise discourse, not unmixed with a touch of juvenile fun. So his days were passed in various studies for his own more perfect culture, and various labours to benefit his fellow-men, and his sun went down amid tender regrets of affection and regard from all his countrymen whom he had loved so well, and who repaid him also with the love which was his due.

James Dwight Dana. By Prof. James Geikie.

(Read July 6, 1896.)

The subject of this notice came of good New England stock, his father, James Dana, having removed from Massachussets to Utica, in New York, where his son was born on February 12, 1813. Young Dana appears to have early indicated a decided taste for the pursuit of science. While a boy he studied chemistry with his schoolmates, and made frequent excursions in search of minerals —a training which, no doubt, was largely instrumental in determining the line of investigation in which he subsequently distinguished himself. At the age of seventeen Dana entered Yale College, where he came under the influence of the elder Silliman, and finally determined to devote himself permanently to science. In 1833 he accepted an appointment as instructor in mathematics to the midshipmen of the U.S. Navy, and while thus engaged enjoyed a delightful cruise in the Mediterranean. Among the fruits of this excursion was Dana's first paper—"On the Conditions of Vesuvius in 1834." His leisure hours on shipboard he seems to have employed in working out, by special methods, certain problems of mathematical crystallography, some account of which he published in the following year. In 1836 we find Dana again at Yale, acting as assistant in chemistry to Prof. Silliman, and busy with the preparation of his first important work, the *System of Mineralogy*,—a volume of 580 pages, which was issued in 1857—surely a remarkable achievement for a youth of twenty-four! Next year he was so fortunate as to be appointed mineralogist and geologist to the Exploring Expedition to the Pacific and Southern Oceans under the command of Commodore Wilkes. The Expedition, consisting of five ships, sailed in August 1838, and proceeded first to Madeira. Thereafter Rio Janeiro was visited, and the ships passed down the coast and through the Straits of Magellan, where one of the smaller vessels was lost in a storm, and the ship to which

young Dana was attached made a narrow escape. He then visited in succession Chili and Peru, and subsequently crossed the Pacific, touching at the Paumotus, Tahiti, and the Navigator Islands, on his way to New South Wales and New Zealand. From New Zealand the voyage was resumed to the Fiji Islands, the Sandwich Islands, the Kingsmill Group, the Caroline Islands, and thence north to the coast of Oregon, where Dana's ship was finally wrecked. He then accompanied the party that crossed the mountains and passed down the Sacramento Valley to San Francisco. Here the wanderers again set sail, and made their way home by the Sandwich Islands, Singapore, the Cape of Good Hope, and St Helena, arriving at New York on 10th June 1842. It is needless to say that the experiences of these four eventful years made a profound impression upon Dana, and influenced all his subsequent life. For the next thirteen years he was fully occupied in studying the materials brought home by the Expedition, and in preparing his reports. His geological observations are contained in a large quarto of 746 pages, accompanied by a folio atlas of 21 plates (1849). Besides this great work, he prepared a *Report on Zoophytes* of similar extent, with an illustrative atlas of 61 plates (1846), and a *Report on Crustacea*, which occupies 1620 pages, and is accompanied by an atlas of 96 plates (1854). Nor were his energies during this period confined to the elaboration of these reports, for we find him at the same time issuing three successive editions of the *System of Mineralogy* (1844, 1850, 1854), and two editions of the *Manual of Mineralogy* (1848, 1857), besides many papers communicated to various scientific journals. Not a few of these appeared in the *American Journal of Science*, of which, in 1846, Dana had been made an editor, associated with Prof. Silliman, whose assistant he had been in 1836–37, and whose daughter he had married in 1842. In 1850 he was appointed to the chair of Natural History in Yale College—the title of the chair being subsequently changed to that of Geology and Mineralogy.

The enormous amount of work accomplished in the few years after Dana's return from abroad testifies to his abounding zeal and enthusiasm. Unfortunately, as his son Prof. E. D. Dana remarks, "he was but little restrained by the thought that injury to health

was possible." But a few years after the last of the Expedition Reports was published his health broke down, and he never quite recovered his former strength. Henceforward, he was subject to the severest limitations as regards work and mental labour; but by avoiding excitement and husbanding his strength, he was enabled to accomplish a wonderful amount of scientific work. Thus, in 1862, he issued his *Manual of Geology*, in 1864 his *Text-Book of Geology*, and in 1868 the fifth edition of the *System of Mineralogy*—his last and most important contribution to that department of science. Notwithstanding all his care, the preparation of this great work proved too much for his strength—his health again gave way, and was only slowly restored. With advancing recovery, he gradually resumed his course of quiet labour—doing much work in the field as a geologist, attending to the duties of his chair, and writing a number of important papers and books. New editions of his *Manual of Geology* appeared in 1874 and 1880, and of the *Text-Book of Geology* in 1874 and 1883. He also found time to write a new work entitled *Corals and Coral Islands* (1872), and yet another geological volume—*The Geological Story briefly Told* (1875).

Dana had so far regained strength in 1887 that he was tempted to take a long journey. The accounts of an eruption of Kilauea in the Sandwich Islands had greatly interested him, and he determined to revisit that region, the acquaintance of which he had first made in 1840. Accordingly, he set out with his wife and youngest daughter, and the result was all that he or his friends could desire. He greatly enjoyed himself, every incident of the visit, his son tells us, being entered into with the enthusiasm of a mind which years could not make old. On his return he wrote a number of papers descriptive of what he had seen, and in the winter of 1889–90 prepared his work on Volcanoes, which, along with a new edition of *Corals and Coral Islands*, appeared early in 1890—the prefaces of both books being dated on his 78th birthday. In the autumn of the same year, however, his health once more gave way, and for several months his busy pen was laid aside. But he could not long endure complete rest; and, by-and-by, was able to dictate a small work dealing with the geology of the New Haven district, which was issued in 1891. His duties as professor

he had no longer strength to continue, but with partial recovery he resumed work on the fourth edition of his *Manual of Geology*. We are told that from this time till the end he seldom worked longer than three hours a day. "To himself, and still more to those about him," his son remarks, "it seemed many times as if the completion of this great work would have to be left to others; but with the self-control born of a strong will and long experience, and with the never-failing watchful care of his life-long companion —without which his labours could never have been so productive, nor have been continued so long—he worked on slowly, doing each day only what he had strength for, and finally the labour was accomplished." He finished it in February 1895. When we know that the volume—a large, closely-printed octavo of 1036 pages—was re-written and re-arranged throughout, and necessarily involved the critical consideration of many new facts, theories, and hypotheses, we shall be ready to agree with his son, that the work is a remarkable performance for a man of eighty-two. He did not even now rest. Work of some kind was for him a necessity of existence. A month after his manual was finished, he had completed the manuscript of a new edition of his *Geological Story briefly Told*, and then commenced work on a new edition of his *Text-Book*. But the end was now at hand. On April 13th he was able to go about as usual, and was as bright and vigorous of mind as ever. In the evening, however, he did not feel quite well, and next morning he did not rise. The uneasy feeling seemed to be passing away, but towards evening it returned, and after a very brief period of unconsciousness he quietly breathed his last.

It is impossible, in a few words, to sum up the results achieved by this laborious and indefatigable student of science. He was an acknowledged master in at least three departments—Mineralogy, Geology, and Zoology; and the broad generalisations which are encountered in his works prove him to have had "a profoundly comprehensive view of nature as a whole." His earliest investigations, as we have seen, were mineralogical—the first edition of his *System of Mineralogy* having appeared so early as 1837. In this work he displayed that anxious desire to do full justice to his predecessors and contemporaries which distinguished his subsequent

labours in this and other fields. In all the later issues of this great work he shows the same astonishing knowledge of the literature of mineralogy. Side by side with this critical compilation of facts, however, we find abundant evidence of independent research and thought. He was at all times less interested in the study of individual mineral species than in the broader questions suggested by a review of the whole science—such as the classification of minerals, theories of crystallogeny, and the morphological relations of species. Mineralogy was his first love, but eventually it became displaced in his affections by its sister Geology. He found in this science greater scope for his activity. The interesting phenomena with which it dealt, and the many problems which it suggested, naturally fascinated a mind like his, and he turned from mineralogy, which he often spoke of as "a department of limited ideas and principles," to devote his best energies to geological investigation. His *Manual of Geology*, which has passed through four editions, has long been recognised by geologists in all countries as a masterly work. It not only sets forth, with admirable clearness, the facts of the science, but everywhere displays the critical acumen, the breadth of view, and originality of a truly philosophical mind. Dana was not only an active and persistent observer in the field, adding much to our knowledge of crystalline rocks and glacial phenomena, but a generaliser of the first order. Hence it is that we find him turning from first to last to such grand questions as the origin of continental areas and oceanic depressions, the problems of mountain-making, and the phenomena of volcanic action. We must remember also that the work he accomplished in Zoology was of great interest and importance. His extensive reports on the Zoophytes and the Crustacea, obtained during the Wilkes' Expedition, contain descriptions of upwards of 700 forms new to science, while his discussion of the relations of species, and his development of the classification, are held in the highest estimation by biologists. Dana began his zoological studies with a belief, then general, in the special creation of species. It was not until many years' reflection that he came eventually to accept the principle of evolution by natural variation. In the last edition of his *Manual of Geology* (1895), he writes : "It is perceived that the law of nature here exemplified is *not* 'like produces like,' but like *with an incre-*

ment or some addition to the variation. Consequently, the law of nature, as regards kingdoms of life, is not permanence but change, evolution. . . . The survival of the fittest is a fact; and the fact accounts, in part, for the *geographical distribution* of the races of men now existing and still in progress; but not for the *existence* of the fittest, or for the power that has determined survival." Again, referring as an example to the giraffe, he remarks that the elongation of the anterior pair of legs has the same purpose as that of the neck—high-reaching in quest of food. "How should the giraffe have had to run to make its fore legs grow faster than the hind legs, and what kind of antics would have started the change in the neck? It has to be supposed that the requisite argumentative variations were somehow begun, and that, under interbreeding, accelerated growth went forward. But the origin of variation is without explanation. And so it is, for the most part, throughout the kingdoms of life. Enough is known to encourage study." Finally, the closing paragraph of his book runs as follows: "Whatever the results of further search, we may feel assured, in accord with Wallace, . . . that the intervention of a Power above Nature was at the basis of man's development. Believing that nature exists through the will and ever-acting power of the Divine Being, and that all its great truths, its beauties, its harmonies, are manifestations of His wisdom and power, or, in the words nearly of Wallace, that the whole Universe is not only dependent on, but actually is the Will of one Supreme Intelligence, Nature, with man as its culminant species, is no longer a mystery."

Although Dana has so recently passed away, it is not too soon to judge of his position in the roll of scientific worthies. The mere mass of facts and data which he has added to the several departments of science in which he laboured would suffice to procure for him a prominent place amongst his fellows. But it is the general suggestiveness of his writings, the originality of his views, and his far-reaching philosophical generalisations, which have most impressed his contemporaries, and which, we believe, will continue to influence his successors. No one who is at all conversant with Dana's work need hesitate to accord him a high place among the

leaders of scientific thought in the century which is hastening to a close.

Personally Prof. Dana was one of the most amiable and genial of men, beloved by his friends, admired and venerated by his pupils. He leaves behind him a reputation of which his fellow-countrymen may well be proud—a reputation which continued to increase up to the very close of his long and fruitful life.

Edward Sang. By D. Bruce Peebles.

(Read May 17, 1897.)

In the latter half of the seventeenth century a farmer of the name of Sang lived in Aberdeenshire, of whom little is known. But we know that he had a family, and that his youngest son, Robert, was born in 1700, and in course of time married Margaret Mitchell, who was an exceptionally clever woman. There is no record of what position he occupied or what business he followed, but very likely it was farming or gardening. He died in 1793, aged 93 years. His son, David, was born in 1749. He was a gardener, and married Mary Chalmers, a daughter of Dr Chalmers of Stonehaven.

Edward, one of his sons, left home when quite young, and settled in Kirkcaldy as a market gardener. He gradually extended his business, and ultimately became a nurseryman and seedsman. In his spare hours he managed by himself to acquire a good knowledge of Greek and Latin, and the difficulties he must have met with no doubt impressed strongly on his mind the advantage of having a good teacher. Business prospered with him, and he married Jean Nicol, a relative of Nicol whose prism is so well known. While giving attention to his business, he must have taken a good deal of interest in municipal affairs, as he was elected Provost of Kirkcaldy, a position he held for a number of years. His son, Edward, our late Fellow, was born on the 30th of January 1805. When he got to the age of seven years it was thought that he should be sent to school. But there were no schools in Kirkcaldy of very high standing at that time, and Provost Sang, along with other gentlemen, started a subscription school in 1812, and engaged Edward Irving as headmaster. After doing so it was natural that they should do what lay in their power to provide pupils for the new school, so we find that three of the Sang family, David, Edward, and their sister, were placed with Irving. The promoters were not wrong in their choice of a teacher, for Irving was a thoroughly able and conscientious

master. In a book lately published, the author, Æ. J. G. Mackay, Sheriff of Fife and Kinross, speaking of Irving, says that "his eloquence, earnestness, and high ideal aims had been known in Kirkcaldy, where it was remembered that he took his pupils to the sands to watch the stars." It may be mentioned, in passing, that in 1816 Thomas Carlyle came to Kirkcaldy, so that we find two men who were to become famous, and a boy who, in his own way, was to rise to eminence, living in close proximity. The boy, young as he was, no doubt felt the influence of his teacher, and possibly of Carlyle also; for two such spirits in a small place like Kirkcaldy could not fail to stir up old and young who came in contact with them, especially those who had any leanings toward science and literature. The boy Sang must have been deeply impressed with Irving's method of teaching, for in after years he used, when occasion served, to give at the meetings of the Royal Scottish Society of Arts vivid descriptions of his school life at Kirkcaldy. He told how Irving made the boys throw down their books and go off with him to field or sea shore to work out problems in land-surveying, astronomy, and navigation. To young and ardent minds no method of teaching could be more fascinating. The pupils were fortunate in having such a teacher. Of the progress the boy made during the first two years of his school life we may judge by referring to a prize he then obtained. We find the following inscribed on a copy of Vince's *Hydraulics*:—

"This book is presented to Edward Sang by the Patrons of the Kirkcaldy Subscription School for his progress in the Higher Class of Mathematics.

(Signed) "EDWARD IRVING."

Sang was not much of a letter-writer, so that there is little in the way of correspondence to weave into such a notice as this. There is one letter, however, the first he wrote, which is peculiarly interesting, as it gives a good idea of what he, a lad of eleven years of age, and his brother, a little older, were thinking about. The letter is addressed, "Mr David Sang, Brucefield, Dunfermline," and is as follows:—

"KIRKCALDY, 7*th December* 1816.

"DEAR BROTHER,—I have sent with the cart the Algebra, Mechanics, and Astronomy. I cannot send the Geometry, Conic Sections, or French Dictionary. I do not know whether you wish me to send any Latin books

or not. You may send word in your next letter. I understand what you mean by some of your work; it was a cissoid. I intend to come at New Year, if I get play for a week, if Father consents. Excuse my letter, being the first I have written.—I am, dear David, your affectionate brother,

(Signed) "EDWARD SANG."

Here we have a boy eleven years of age—an age at which many boys are scarcely out of the nursery—getting a prize for his progress in the higher class of mathematics, and evidently busy with mechanics, astronomy, French, and Latin. Continuing his school life, and no doubt working hard, he obtained another prize in 1817, on which was inscribed:—

"This book was presented to Edward Sang by the Patrons of the Kirkcaldy Subscription School as a reward for his diligence, and a testimony of his success in the study of Mathematics during the Session that closed this day, the 1st November 1817. (Signed) "EDWARD IRVING."

The book was Legendre's *Eléments de Géométrie.* The boy was then twelve years of age. Before leaving the story of his school life, a short extract from a letter written by the Rev. Dr Martin a number of years after may be read. He says:—"Mr Edward Sang has been known to me from his infancy. He began very early to show an uncommon inclination toward mathematical science, and a peculiar aptness for it. As an amateur of that science, I had my attention drawn to his talent for it while he was, with some of my own family, a pupil in an academy in this town in which mathematics were then taught with remarkable success. I still remember the surprise excited by the acute and comprehensive solutions he gave of the problems, theorems, and questions presented to his class."

The Subscription School was broken up in 1818, and in the first year of his teens the boy joined the University of Edinburgh under Professor Leslie, and had to take the second class for mathematics, there being no third or advanced class for that session. He was small for his age, and on his appearance in the class-room he was greeted with laughter by his fellow-students, big fellows, who no doubt wondered what such a youngster was to do in such a class. But the laughter soon gave place to surprise and admiration. Next session there was still no advanced class, and he had again to take the second mathematical class, under Professor Wallace. During

the next four sessions he studied Natural Philosophy under Professor Leslie, two as a regular student and two as a holder of a perpetual ticket. His career in the University was one of uninterrupted progress, and on its termination he received the following certificate:—

"College of Edinburgh, 20*th April* 1822.—I hereby certify that Mr Edward Sang has most regularly attended the Natural Philosophy Class during the whole of the Session now closed, that his application was ardent and unremitting, and the talents, ingenuity, and penetration which he displayed place him decidedly above all his fellow-students.

(Signed) "John Leslie."

After leaving college he commenced and continued for some years the practice of surveyor and civil engineer, and then became a teacher of mathematics in Edinburgh. In 1828 he was elected a Fellow of the Royal Scottish Society of Arts, and during his long connection with that body he brought before it some of his most valuable papers. We have here a list of his writings, 112 in number, on a great variety of subjects connected with Mathematics, Natural Philosophy, Horology, Astronomy, Engineering, etc., besides his great work on Logarithms. A glance over these gives one an idea of the diversified character of his studies, and suggests the query as to whether his work as a whole would not have been more valuable had he confined himself to a few, instead of dealing with so many subjects. It has been said that there is plausibility in asking, "not if a man can do many things well, but if he has done one thing supremely." There are some minds—minds of a high order, too—fitted to attack and stick to special work, and it would be a wonder and a disappointment if their work was not supreme. Sang was not one of those; nevertheless, he did many things supremely, but could not be a specialist. The variety of his papers shows that clearly; and while there is no time even to read over their titles, a few may be noticed in their order as having been received with marked favour and approbation. In 1829 he published a small work containing an account of a new method of solving numerical equations, the first of the many works that came from his hands. About 1830 Professor Wheatstone exhibited a very beautiful series of curves, produced by fixing a polished ball on the end of a wire and causing it to vibrate. This

was shown in Edinburgh, and attracted Sang's attention. He went into a consideration of the subject, and made the discovery that every wire, no matter what may be the form of the hole through which it has been drawn, has one direction in which the flexibility is greatest, and another, at right angles, in which the flexibility is least. He tells that he was startled by finding that theory led him to believe that, if the rapidity of vibrations in the one direction be double of that in the other, the common parabola should be the result. He then drew out curves according to theory, and manufactured wires of the proper proportions as indicated by the theory, and found a perfect coincidence between the actual and computed phenomena. In 1831 he exhibited before the Royal Scottish Society of Arts these wires, with their silver knobs, and the beauty of the curves was much admired. They never ceased to charm him, and he often brought them out to show to friends. In 1889, after a lapse of fifty-eight years, he again brought the subject before the Society of Arts, and exhibited a new set of wires he had recently made. In 1838 he read a paper to the Society of Arts, describing a Dioptric Light erected at Kirkcaldy, and he also gave a description and exhibited drawings of the apparatus he used for cutting the annular lens to the true optical figure. For that paper he was awarded the Keith Medal, value 20 sovereigns. In presenting the medal, Sir John Graham Dalziel stated that "no opportunity had hitherto occurred since the Keith fund came into the possession of the Society for awarding that prize. Now, however, it was highly gratifying to find one of the most scientific, useful, and meritorious of all who had been connected with the Society, Mr Edward Sang, entitled to this eminent distinction. His skill, his labours and unremitting exertions in various scientific departments, were too well known to be embellished by any commentary." In 1840 he was presented with the Society of Arts Silver Medal, value 10 sovereigns, for his papers on the "Construction of Circular Signal Towers," on the "Effects of the Curvature of Railways," and for his valuable essays on Life Assurance. For a period of several years he lectured on Natural Philosophy, and in 1841 he became Professor of Mechanical Science in the Manchester New College. Shortly after, he went to Constantinople to assist in establishing schools of civil engineering, and in laying out railroads

in Turkey. He assisted in the erection of ironworks at Zeïtun Buruni, and was afterwards engaged in several colleges completing the courses of education. In addition to this work, he proceeded to compile treatises on the Method of Co-ordinates, on the Differential and Integral Calculus, on Mechanics, Hydrostatics, and the Elements of Physical Astronomy. This arduous task was accomplished by means of oral lectures in the Turkish language, of which full notes were taken by the pupils, and these notes were extended by the students and compared. By this means accuracy of idiom was secured, and the precision of the technical terms taken from the Arabic was ascertained.

Sang never let an opportunity slip of helping truth or correcting error, and he took advantage of an approaching event which led to this, and also gave him a chance of dispelling fear and removing superstition. The solar eclipse of 1847 was close at hand, and was to be annular, and almost central as seen from Constantinople, so he computed its details with great care, and prepared large drawings, with a descriptive notice in French. These were presented to the Sultan through his Excellency Lord Cowley. Notices were also given in the Turkish and French newspapers, accompanied by lithographed drawings on a smaller scale, while prints were freely distributed with an explanation in Turkish. With a great roll of these prints under his arm for distribution, Sang often went to out-of-the-way places and into the bazaars. The priests were the worst to deal with, and were most unwilling to touch the prints, saying that no man should meddle with such matters, as they belonged to God alone. But many of them listened to reason, and a little sensible talk helped generally to overcome their scruples. The excitement was great, and the preparation of pieces of smoked glass was carried on most vigorously by large numbers of the community as the predicted time of the eclipse approached. In those days the uneducated classes looked upon eclipses with terror and consternation, as in many places they still do. There are few amongst ourselves, even now, who are free from awe and wonder when there is a total eclipse, and the weird and unnatural darkness sends bird and beast into seclusion, and brings about a deathlike silence.

The Sultan had made great preparations for the coming event.

With telescope and chronometer at hand, he waited for the second which Sang foretold that the commencement of the wonderful phenomenon would take place. And, sure enough, the very moment which was predicted saw the beginning of the interference with the sun's light. The firing of guns, the blowing of trumpets, the beating of drums and shouting now began, that being thought the only way to bring back the sun's light and prevent the disasters which it was believed would certainly follow. The Sultan at once sent out his officers to stop all that, and the end of Sang's labours was accomplished by the dispelling of superstition and the quelling of the alarm which had always accompanied the natural but uncommon deviation from the daily gradation of light and darkness. This was a triumph of science which had a lasting effect on the population of Constantinople, for it not only removed in a great measure the superstition of the people, but also excited and stimulated the students to such a degree that it led to the details of the next eclipse of 1851 being computed and prepared by Sang's fifth class, at the Imperial College, Muhendis-hana Berri.

In 1849 he was elected a Fellow of this, the Royal Society of Edinburgh, and there were few, perhaps, who took a keener interest in its proceedings. In 1851 he was invited by a circular of the British Association, sent at the instance of the secretary of the Royal Scottish Society of Arts, Edinburgh, to proceed to Russia for observing the total eclipse of the sun. The shortness of the notice, and the calmness of the weather during the voyage, prevented him from reaching farther than Sevastopol, where he arrived just when the eclipse began, so that he could do nothing; and the only result of the trip was to intensify a longing to return to Scotland. There are few Scotsmen who go abroad but are at some period of their exile seized with nostalgia, and sometimes in such a fashion as not to be conquered. It was so in this case. He resigned his situation, greatly against the wish of Fethi Pasha, who would not give his formal consent. Nevertheless Sang returned to Edinburgh in 1854, and resumed his former occupation as a teacher of mathematics.

During his residence in Constantinople he won the respect and esteem of all the various nationalities with whom he came in contact, notwithstanding the differences of religion and race, and

amongst the Turks his name to this day is held in reverential remembrance in the colleges in which he was professor. In 1889, shortly before his death, a young Turkish gentleman being in this country, could not leave it without finding out and coming to see the *hodja*—the teacher—of whom he had heard so much, and who was always spoken of with admiring respect and affection.

In 1856 he read a paper on the Gyroscope in relation to his suggestion of a new experiment which would demonstrate the rotation of the earth, and he claimed that he had clearly proved this by experiment in 1836,—eighteen years before Foucalt performed his experiment at the meetings of the British Association in Liverpool. In connection with this, Professor Baden Powell, in a letter to Professor Piazzi Smythe, says:—" I have just received, I presume through you, a copy of Mr Sang's paper on 'Rotation.' Pray thank him from me if you have an opportunity. It is extremely interesting to find how completely he anticipated the idea so long ago." Professor Chevallier also wrote to Smythe, saying:—" If poor Arago were still alive he would, as a Frenchman, feel himself 'like a woodcock caught in his own springe,' for he always held that a paper communicated to a recognised public scientific society, and regularly entered in their Proceedings, was to all intents and purposes a publication to the world of an invention. Mr Sang must be no ordinary man to have conceived so clearly the solution of so difficult a question by mechanical means. I hope that his claims will now be made generally known."

In 1862 Provost Sang, one of the promoters of the Kirkcaldy Subscription School, died, aged 91 years.

In 1879 the Institution of Civil Engineers, London, awarded to Sang the Telford Premium for his paper on " A Search for the Optimum System of Wheel Teeth." The paper contains elaborate and intricate calculations, undertaken with a view to discover the best form of wheel teeth to adopt as a standard, in order to avoid the lack of uniformity which had so long existed. Another important paper was read by him to the Society of Arts in 1861 on " The Determination of the Form of a Ship's Hull by means of an Analytic Expression," its object being to improve naval architecture and substitute a scientific method, instead of rule-of-thumb and guesswork, in hull construction The Society referred the paper to

a committee, which reported most favourably upon it, stating that "the author deserved great credit for the analytical skill and ingenuity he had displayed in the investigation, which must have cost him much thought and great labour in computing from their equations, and tracing a large number of curves, in order to obtain the requisite familiarity with the use of the formulæ, and also for the purpose of constructing the model which was shown." This paper was awarded the highest prize the Society could give, viz., the Keith Prize, value 30 sovereigns. On the 8th December 1873 he read a paper giving a description of a new machine for the hand-spinning of rope yarn. The machine was shown in action, and the work performed by its beautiful compound motion was much admired. Any length of rope could be spun without having recourse to the long rope-walks commonly used. In 1886 this Society presented him with the Macdougall-Brisbane Prize for 1882–84 for his paper "On the Need for Decimal Sub-Divisions in Astronomy and Navigation, and on Tables therefor."

Our learned Fellow, Lord M'Laren, who was in the chair, in making the presentation, said that "Dr Sang's paper covers a wide range of inquiry, embracing various branches of pure mathematics, mechanics, and optics, as well as the applications of these sciences to practical astronomy, chronometry, and naval architecture. No considerations, save zeal for the advancement of science and a benevolent desire to lighten the labours of future computors, could have induced Dr Sang to undertake such a gigantic task, or have sustained him through the wearisome mass of mechanical detail which overlaid the more interesting parts of his occupation." Time will not permit of noticing more of his work, a fraction of which has only been touched upon, but enough to show what the man was and what he did. Everything he gave his mind to as a philosopher was undertaken with an honest, conscientious, and single-minded devotion to science; and everything he put his hand to as a craftsman was marked with a beauty of design, a faultless precision and delicacy of finish, of which the most skilled workman might be proud. It may be added that accuracy in workmanship was insisted on by him to an extent that to many in times past seemed useless. In this he surely anticipated what was coming, for the marvellous development of automatic machinery—

especially in America, in which accuracy is a *sine qua non*—shows how true his instinct was, and how correct his views were regarding the designing and construction of tools. Even in small matters his love of accuracy crops up, for he had his drawing-pens fitted with screws having divisions on their milled heads representing lines of different breadths, to which the pen could at once be set. He was a beautiful draughtsman, and was never at a loss, with pen or pencil, in making clear even complicated pieces of mechanism.

The man of science goes to nature and asks pointed questions. To these, up to a certain limit, he gets answers of precision; but philosophers are not inclined to stop there, and they go on asking questions to which they can get no possible solution. Why? is ever on their lips; and when there is no answer, or one which is unsatisfactory, belief in the existence of a Supreme Ruler and Governor is apt to vanish, and Doubt sits down in the empty chair from which Faith has been driven. Sang was not one of that class. He never obtruded his opinions on such matters, but his belief in a Supreme Ruler was strong and unequivocal. If he alluded to the subject it was always in the most reverential spirit. In an address he gave at the Jubilee of the Society of Arts in 1867, he said:—"The exquisitely-carved shell of the minutest diatom reveals arrangements and contrivances infinitely beyond all that man has done or ever will do; and we place our hands upon our mouths, our faces in the dust, in the presence of a wisdom that we cannot begin to comprehend, of a goodness that overwhelms us." In a paper he read in 1884 he concluded by saying:—"Of the untiring goodness, the unfathomable wisdom evident in all that passes around us, even in the mysterious complexity of human life, let us recall what has been said,—'He rewardeth the searcher and the keeper of His laws.'" On a large telescope which he mounted himself for the Wray lens he got from the Institution of Civil Engineers, he inscribed that precious motto in golden characters in Turkish and English; and when, in times of depression, from which few are exempt, he felt that his labours were not rewarded as they should have been, we can fancy him getting comfort and inspiration from such a motto. But he was a busy man, and the busy man is generally a happy man. Reason rather than memory was what he valued and appealed to in connection with his pupils. Parrot-work

he hated, and the palaver of a dilettante he met with ridicule. He received a grant of £100 per annum from Government as a recognition of his valuable scientific work; and the associated Scottish life assurance offices, feeling that some substantial recompense was due to him for his logarithms and actuarial tables, at a meeting in 1878 resolved to recommend to the offices the payment of an annuity of £100 . . . . for the remainder of his life, which was agreed to and subscribed by the offices. His great work on Logarithms, faultless as it is believed to be for accuracy, is a monument not only to his mathematical skill, but to his tenacity of purpose and love of science. There are few who for forty years could have, with what may be termed intermittent continuity, persevered with such a colossal work; and the pity of it is that forty-seven volumes of such valuable matter for astronomers, navigators, and others should be lying uncared for and useless. Such a work surely demands the care of Government or some of the learned societies. In 1881 he was made corresponding member of the Royal Tunis Academy. In 1883 he was honoured by being made an LL.D. of Edinburgh University, and in 1884 he was made an honorary member of the Franklin Institute, Philadelphia. In 1889, feeling his advanced age telling upon him, he resigned the post of secretary to the Society of Arts; and his last paper to this Society, on "The Extension of Brouncker's Method," was written in 1890, and read by Professor Tait on the 15th December. For some months before this he had been failing in bodily health, but his mind was clear and undisturbed. Four days before his death he dictated letters, and also wrote some himself; but the end was near, and on the 23rd of December he died, within a few days of reaching the age of 86 years. A long-lived race were the Sangs—Robert reached 93 years, David 88, Edward of Kirkcaldy 91, and Edward, our late Fellow, 86, giving an average of close on 90 years for the four generations.

The mourners, who were honoured in having been chosen by himself to attend and pay the last tribute of respect at his funeral were all sincere friends, who grieved over the loss they had sustained in the departure of one whom they loved and deeply respected, not only as a man, but also for his great and varied learning.

Few have lived with such an enthusiastic and single-minded

devotion to science, and his rewards in a worldly sense were far from commensurate with his great and valuable labours. In writing this paper a free use has been made of the *Transactions of the Royal Scottish Society of Arts*, a short autobiography, and information kindly supplied by the Misses Sang. It may fitly be closed by a quotation from one of Miss Sang's letters. Speaking of her father, she says:—" To investigate, to endeavour if possible to reach the fundamental principle, and so be able to build a firm superstructure on a sure foundation, was a passion with him in which he found a happiness few could realise, and he was always ready to communicate to others from his own stores." Let us hope that the promise of his favourite motto is being amply fulfilled in another sphere, where he now finds how true it is, with reference to a Supreme Ruler, that " He rewardeth the searcher and the keeper of His laws."

LIST OF WRITINGS.

1. "Solution of Algebraic Equations of all orders, whether involving one or more Unknown Quantities." Edinburgh, 1829.
2. " On a Remarkable Analogy between the Primitives and Derivatives of the Product of two Monome Functions." *Annals of Philosophy*, August 1829.
3. " Observations on the Theory of Capillary Action given in the supplement to the 'Encyclopædia Britannica.'" *Edinburgh Philosophical Journal*, February 1830.
4. " On the Adaptation of the Fly-wheel and Pully of the Turning-Lathe to a given length of Band." April 1831.
5. "Experiments made to Determine the Thermal Expansion of Marble." June 1831.
6. " A New Solution of that case in Spherical Trigonometry in which Two Sides and the Contained Angle are given." 1832.
7. "Analysis of the Vibration of Straight Wires." *Edinburgh Philosophical Journal*, April 1832.
8. " A few Remarks on the Relation which subsists between a Machine and its Model." November 1832.
9. " Remarks on some Prevailing Misconceptions concerning the Actions of Machines." January 1833.
10. " On the Advantages of a Short Arc of Vibration for the Clock Pendulum, with a Table of Corrections of the Daily Rate." July 1833.

11. "Meteorological Observations made at Edinburgh during the Solar Eclipse of 7th July 1833."

12. "A Method of Freeing the Determination of the Latitude of an Observatory from the consideration of Atmospheric Refraction." August 1833.

13. "First Essay preliminary to the series of Reports ordered by the Society of Arts for Scotland." September 1834.

14. "On a certain Relation between the Successive Prime Numbers." Laid before the British Association.

15. "The First Book of the Geometry of Sines of the Third Order." Laid before the British Association.

16. "Second Essay preliminary to the series of Reports ordered by the Society of Arts, Scotland." October 1834.

17. "Report on the Recent Improvements in the Carpet Manufacture." August 1835.

18. "On the Manner in which Friction affects the Motions of Timekeepers." July 1835.

19. "Suggestion of a New Experiment whereby the Rotation of the Earth may be Demonstrated." January 1836. Read before the Society of Arts, 9th March 1836.

20. "Account of an Improvement in the Construction of Wollaston's Goniometer." Society of Arts, 1836.

21. "Annual Report on the State of the Useful Arts." Read before the Society of Arts, 7th December 1836.

22. "On the Construction of a Solid Achromatic Eyepiece." Royal Society, 6th January 1837.

23. "On the Construction of Eyepieces for Transmitting only one of the Pencils of Polarised Light." 30th January 1837.

24. "Second Report on the Progress of Exactitude in the Manufacture of Machines." June 1837.

25. "Notice of Precautions to be taken while using Mr Adie's Anemometer." Society of Arts, January 1838.

26. "Notice of a Method of Determining the Velocity of the Wind." Society of Arts, 10th January 1838.

27. "Notice of a Singular Phenomenon connected with the Rotatory Motion of Fluids." Society of Arts, 14th February 1838.

28. "Description of the Phoroscope, an instrument for Measuring Time and Velocity." Society of Arts, 28th February 1838.

29. "Description of an Improved Nut for Leading Screws." Society of Arts, 28th March 1838.

30. "Notice of a Dioptric Light erected at Kirkcaldy Harbour, with a Description of the Apparatus used in Making the Lens." April 1838.

31. "Theory of the Construction of Oblique Arches." May 1838.

32. "Notice of an Erroneous Method of using the Great Theodolite, practised by the Ordnance Surveyors, with a Strict Analysis." November 1838.

33. "On a Method of obtaining the greatest possible Exactitude from the Data of a Survey." January 1839.

34. Description of a new Waving Bar for Engravers' Ruling Machines." Society of Arts, 30th January 1839.

35. "Essay on the Law of Mortality in England and Wales, deduced from the Return by the Registrar-General." December 1839.

36. "Essay on the Bonus System of Life Assurance Offices." January 1840.

37. "On what Constitutes the Profits of a Mutual Assurance Society, and on the only Equitable Method of Distributing these among its Members." January 1840.

38. "On the Money Values deduced from the various Bills of Mortality." March 1840.

39. "On the Construction of Circular Signal Towers." April 1840.

40. "On the Effects of the Curvature of Railway." May 1840.

41. "On the Nodus Rosi (a Phenomenon Exhibited by some Specimens of Calcareous Spar brought from Iceland by Mr Rose)." Society of Arts, 8th March 1841.

42. "On the Proper Form for the Convertible Pendulum." July 1841.

43. "On an Erroneous Deduction from Captain Kater's Experiments on the Flexure of Thin Bars." April 1841.

44. "On a Method of Registering the Force transmitted by a Driving-Belt."

45. "Life Assurance and Annuity Tables—One Life in Folio." Edinburgh 1841.

46. "Delineation of the Annular Eclipse of the Sun as it will be seen from Constantinople on Saturday, October 9th, 1847." May 1847.

47. "Account of Observations made at Sevastopol on the Solar Eclipse of July 18th, 1851." Read before the Royal Scottish Society of Arts, 24th November 1851.

48. "A New General Theory of the Teeth of Wheels." Edinburgh, 1852.

49. "Description of a Chronofore or Hackwatch for Measuring to Minute Intervals of Time." Society of Arts, 27th November 1854.

50. "Theory of Driving-Belts." Society of Arts, January 1855.

51. "On an Improved Mode of Constructing Standards of Weight." Society of Arts, December 1855.

52. "On an Inaccuracy (having its greatest value about 1″) in the usual Method of Computing the Moon's Parallax." Read before the Royal Society of Edinburgh, 19th February 1855.

53. "On the Accuracy attainable by Multiplied Observations." Read before the Royal Society of Edinburgh, 30th April 1855.

54. "Geometry is a purely Experimental Science." Read before the Royal Society of Edinburgh, 7th January 1856.

55. "On the use of the Altitude and Azimuth Circle for Stereometric Surveying." Society of Arts, January 1856.

56. "On Turkish Weights and Measures." Read before the Royal Society of Edinburgh, 4th February 1856.

57. "Theory of the Free Vibration of a Linear Series of Elastic Bodies." Read before the Royal Society of Edinburgh, 18th February 1856, and published in the *Edinburgh Philosophical Journal.*

58. "Elementary Arithmetic." W. Blackwood & Sons, 1856.

59. "The Gyroscope." 1856.
60. "Higher Arithmetic." W. Blackwood & Sons, 1857.
61. "On the Exhibition of Both Roots of a Quadratic Equation by One Series of Converging Fractions." Read before the Royal Society of Edinburgh, 18th January 1858.
62. "Life Assurance and Annuity Tables, for every Combination of Two Lives." Vol. ii. 1859.
63. "Pendulum." *Encyclopædia Britannica*, vol. xvii.
64. "Perspective." *Encyclopædia Britannica*, vol. xvii.
65. "Saw and Saw Mill." *Encyclopædia Britannica*, vol. xix.
66. "Skew Bridge." *Encyclopædia Britannica*, vol. xx.
67. "Determination of the Form of a Ship's Hull." 1861.
68. "Sefinet Equation for Determining the Form of a Ship's Hull." 1863.
69. "Catadioptric Altitude and Azimuth Circle." 1862.
70. "Deflection of Plummet." 1862.
71. "Roots of Equations." 1863.
72. "Roots of Equations." 1864.
73. "Crystal Pointer Clock Adjustment." 1864.
74. "Commensurables." 1864.
75. "Motion in a Circle." 1865.
76. "Epicycloid." 1865.
77. "Recurring Functions." 1866.
78. "Brouncker's Method." 1870.
79. "Motion in a Circle." 1870.
80. "Account of New Table of Logarithms to 200,000." 1872.
81. "Crystal Cavities." 1873.
82. "Rope Yarn Machine." 1873.
83. "Canon of Sines." 1877.
84. "Revolving Wire." 1877.
85. "Unround Discs." 1877.
86. "Wheel Teeth." Part ii. 1877.
87. "Ballistic Curves." 1878.
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89. "Approximating Fractions." 1878.
90. "Optimum Wheel Teeth." 1879.
91. "Noveau Calcul." 1879.
92. "Addition on Noveau Calcul." 1880.
93. "Comet." 1881.
94–5. "Solar Eclipse—Mirage." 1882.
96. "Time of Descent in a Circular Arc." 1882.
97. "Aerial Images." 1883.
98. "Nodus Rosi." 1883.
99. "Simple Flexure." 1883.
100. "Cubic Equations." 1884.
101. "Log* Sines (on the Construction of the Canon of)." 1884.
102. "Lathe Band." 1884.
103. "Decimal Sub-Division." 1884.

* "Trigonometrical and Astronomical Calculations, 47 vols.

104. "Chain Pendulum," 1887.
105. "Instability in Open Structures." 1887.
106. "Eclipse of the Moon." 1888.
107. "On Leslie's Computation of." 1888.
108. "Compensation Balance." 1888.
109. "Compound Goniometer." 1889.
110. "Air's Resistance." 1889.
111. "Curves of Vibrations of Straight Wires." 1889.
112. "Extension of Brouncker's Method." 1889.

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